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Articles to be Published in Early Numbers

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Sterility in the Female: Report of Cures (Operative)—Charles C. Child, New York, N. Y.

Gynecological Problems in Industrial Medicine—Harry E. Mock, Chicago, Ill.

Hemorrhages into the Pelvic Cavity Other Than Those of Ectopic Pregnancy.—Richard R. Smith, Grand Rapids, Mich.

The Development of Prenatal Care and Maternal Welfare Work in Paris Under the Children's Bureau of the American Red Cross.—Fred L. Adair, Minneapolis, Minn.

The Importance of a Follow-Up System for Obstetric Patients.—Geo. W. Kosmak, New York, N. Y.

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Operation or Radium for Operable Cases of Cervical Cancer.—William P. Graves, Boston, Mass.

An Analysis of the Failures in Radium Treatment of Cervical Cancer.—Frederick J. Taussig, St. Louis, Mo.

Lutein Cysts Accompanying Hydatiform Mole.—W. A. Coventry, Duluth, Minn.

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A New and Modern Treatment of Cancer of the Breast.—James F. Percy, San Diego, Cal.

Some Indications for Hysterectomy.—James F. Baldwin, Columbus, Ohio.

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Enuresis.—John W. Keefe, Providence, R. I.

The American Journal of Obstetrics and Gynecology

VOL. I. ST. LOUIS, OCTOBER, 1920 NO. 1

Announcement

THE editors and publishers have the honor to present the AMERICAN JOURNAL OF OBSTETRICS AND GYNECOLOGY to the medical profession for its approval.

The importance of obstetrics and gynecology as an integral part of medical art and science should be measured not only by its interest to those directly engaged in it as a specialty but likewise to those who practice medicine in a more general sense. A topic that necessarily commands the attention of so many physicians must be adequately represented in journal literature and demands a medium of publication primarily devoted to its advancement and welfare. The present venture has been developed in response to this need.

The new AMERICAN JOURNAL OF OBSTETRICS AND GYNECOLOGY will be conducted by the profession, for the profession, and in order that the latter may be definitely represented in its management, the general conduct of the enterprise is vested in an advisory board, the membership of which serves as a guarantee that the interests of the profession will be adequately protected and assured.

The policy of the AMERICAN JOURNAL OF OBSTETRICS AND GYNECOLOGY will be liberal in the sense of service to the specialist in this field, as well as the general practitioner. The clinician, the pathologist, the research worker, and the sociologist are all welcome to express their thoughts in its pages, which we trust will serve as an effective medium of exchange for the many whose united efforts contribute to make obstetrics and gynecology one of the essential branches of medicine.

In addition to original communications and records of special society transactions, a feature in the new journal will be made of the department devoted to current medical literature. This important department will be conducted by the Associate Editor, Dr. Hugo Ehrenfest, who will present recent progress and research in this field in the form of carefully prepared reviews and criticisms. We believe that this arrangement will be of great value to our readers.

The editors and publishers respectfully submit the AMERICAN JOURNAL OF OBSTETRICS AND GYNECOLOGY to the American medical profession, fully realizing the trust imposed upon them and hoping for an endorsement of the attempt to serve its interests.

Original Communications

A PROGRAM FOR AMERICAN GYNECOLOGY
PRESIDENTIAL ADDRESS
AMERICAN GYNECOLOGICAL SOCIETY*

By Robert L. Dickinson, M.D., F.A.C.S., New York, N. Y.

OPPORTUNE and insistent, in these days of analysis and readjustment, there stand out, in our Constitution and in our motto, two special phrases. "The object of this Society," says the former, "shall be the promotion of knowledge in all that relates to the Diseases of Women, to Obstetrics and to Abdominal Surgery." Our motto may be rendered: "Like the stars, each revolves, without haste, without rest, round his own particular task." "All that relates," says one; "his own task," says the other.

SURVEY

In earlier years the urgency that was inherent in certain parts of the work put off the balanced promotion of the whole, and it was also inevitable that tangents should be struck off from our own orbit. But now it is meet and right, as several authors in the past year have urged, that we diligently examine ourselves whether our own omissions have had anything to do with such situations as an untouched obstetric mortality and morbidity and the threat of eclipse of the gynecologic guild. Surgery we promoted. But if we be just surgeons, by surgeons we may be displaced. Diagnosis we developed and principles we have taught, till all may follow—and supplant. The point is come where old fields must give new crops and new lands be opened up or our claims surrendered. The war searched out most of the barren spots in medicine, and the geography of the spots. In that search the Council of Defense and then the Army, sifted the men and the methods of every specialty save one. Our group-work, useless to war, escaped ordered scrutiny, and now, after the war, presents the anomaly of being without an inventory and lacking in data on which to construct a plan for intensive culture. Some material is provided, of course, in the studies of standards in colleges and hospitals made by the American Medical Association, the American College of Surgeons, and the Carnegie investigations. Further studies into the various departments of medicine are imminent. It is for us to decide whether we shall lead or be led in such surveys. Now, we see, as in a speculum, darkly. It is for us to say how we wish to be seen in the future.

*Read at the Forty-Fifth Annual Meeting of The American Gynecological Society, Chicago, May 24-26, 1920.

EXTENT OF THE FIELD

There are reasons why our specialty should readily lend itself to this scrutiny. It presents the paradox of being limited, yet large. The total list of disabilities is brief compared with general surgery—the actual incidence is surprisingly large. Gynecologic operative procedures comprise one-fourth of the surgery of sorts. The figures on which the above statement was made have been derived from a careful review of notices of operation issued daily by our Clinical Congresses and by the Society for the Advancement of Clinical Study of New York.

One fourth of surgery. Yet this does not indicate the extent of the field, since operation is needed by less than one tenth of the patients that come to the doctor for ailments peculiar to women (childbearing not included). Even this last figure will alter greatly, though lowering the operative percentage when thorough routine preventive examinations shall prevail. Thus, whether for sheer bulk, or for need of skill of scalpel, or for setting a pace for humane handling of the human personality, our field may claim to be without parallel in opportunity.

NEW WORK

In searching for the gaps in the promotion of knowledge in our task, the questions naturally arise: Are there problems which the man in our line is alone qualified to solve? Are there studies or pronouncements that such a society as this is particularly equipped to undertake? An affirmative answer would include such subjects as the following paragraphs take up, though one cannot presume to make more than a sketch of the whole area.

NOMENCLATURE

A standard nomenclature should be a matter of agreement on the part of authoritative organizations. At present we find ourselves unable to place in parallel columns the results of writers and institutions in general. A new start has been made. All the great groups involved were invited to unite in action and to come together on one list centering in a government office. The Census Bureau has just issued such a volume. It is evidence that the Bureau no longer confines its interests to the defunct. This "Standard Nomenclature of Diseases and Pathological Conditions, Injuries and Poisonings for the United States" is submitted to you by your president, the chairman of the original committee. It is submitted for criticism and suggestion as a first step—and a first step only—toward ultimate agreement, in the hope that we will amend this edition and care to keep an active representative on the main committee. The hearty coöperation and representation of the Army, the Navy, and the Public Health Service, the various national medical organizations, the large insurance companies, and all factors having to do with vital statistics make this volume representative as no previous attempt in any language has ever been representative.

OPERATION NOMENCLATURE

To supplement and complete this action there should be produced an acceptable list of operative procedures. Your presiding officer submitted to some

of you several years ago such a working list. He is glad to find a member noted for thoroughness newly laboring on this set of terms. Without this standard no two operators can be sure they are comparing the same processes, the variant use of the words "total hysterectomy" being an instance in point.

DEFINITIONS

While we are delimiting terms there are not a few that appear in every hospital report that make for confusion, for which exact bounds could be set. Among these are the words "major" and "minor" as applied to operations. We might even find or invent a name for a member of our double or bracketed specialty. We might define the qualifications for membership in this Society. Such a statement of a standard does not exist.

STANDARDS

This Society might take upon itself to establish certain standards in obstetrics and gynecology. Just what is meant by "standards?" I take it to be the best present practice, widely studied, fairly epitomized, succinctly written down, warily applied. Though concrete, it is not set like cement, as some assume. Rather does it alter as it alteration finds, rising to meet each step of well attested progress, sanely adapting its content to each special group of conditions, but never falling below a reasonably high requirement. Says Dr. George E. Vincent of the Rockefeller Foundation, "It is well to recognize the relativity of ideals. In human institutions there are no absolute standards." He goes on to give the warning that while the highest ideals may be approximated in a few institutions it "would be a serious mistake not to recognize various degrees of achievement." "All that can be confidently claimed is that some * * * fall far below any standard that can be recognized as guaranteeing results which will safeguard the public and protect the profession." "There is danger that the existence of a standard may force a formal rather than a real compliance with ideals," and "a premature effort to conform to a standard may do actual harm." However, this may be—and it applies equally to detail of everyday procedure as well as to great issues—there can only be profit in presenting ideals. This, as the College of Surgeons shows us, is found in actual practice to be the essential step, and in most cases all that is needed. The competitive instinct does the rest. If we, for instance, were to place side by side the product and practice of a certain finely equipped northern clinic and that of one small southern service that steadily breeds teachers, our Society would be fulfilling its function of promoting knowledge concerning obstetrics—in the minds of one set of trustees. The publication of the average necessity for Cesarean compared with its frequency in one of our clinics had only to be stated to bring strong condemnation. The Society can get publicity where individuals could not. We can report on ourselves, and on our own communities. As Henry S. Pritchett said in the last Carnegie report on teaching, "There could be no more wholesome contribution to education than to ask our universities to take stock of themselves. No process could be more helpful than to ask the governing bodies

of these institutions to render a sincere and critical statement of the results obtained in the last 25 years." We may well do this for our department. And we may do another service in this line that would initiate a most wholesome contribution to surgery. This service would consist in taking stock of unnecessary operating. Such stock-taking should begin with our own clinics, for the least excusable of unnecessary suffering is that on women—and particularly on mothers of young children.

AXIOMS

Even though for the present any general acceptance of certain items of betterment in practice may seem unlikely, forward progress should initiate with us and not be imposed upon us by the demand of public opinion. Advocacy of axioms like the following cannot fail to bring about greater confidence in the profession on the part of the laity: "Except in emergency, no operation without consultation, medical and surgical." The chief will himself set the example, and see to it that the conferences will not be merely perfunctory.

"The findings at consultation set down in writing."

"Every patient discharged after operation to be provided with a written statement of the postoperative diagnosis and the operative measures."

"A patient (of office, hospital or sanitarium) is not getting fair or full value for her fee who does not receive a statement in writing—a diagnosis when provisionally or definitely made—a summary of treatment found to be successful or to fail."

"No woman may be examined unclothed or have a pelvic examination made (except in emergency) without the presence of a third person."

SOCIOLOGY AND GYNECOLOGY

The Society's interest in sociologic problems, to judge from recent volumes of the Transactions, does not evince itself to the extent of half a dozen papers in a dozen years. These are of a limited range, dealing chiefly with venereal diseases and prenatal care. Henry Newman drew our attention to the need of action last year. Our final act was to empower the Council to promote this movement. This promotion should be by action rather than words. Tonight we begin, by getting advice from authorities on the subject. Geddes, physician and ambassador, deplored the fact that doctors in general lack the spirit of citizenship. Otto Geier, prophet of industrial medicine, has drawn attention to the absence of medical men on the recent great commissions. We, a hundred and fifty thousand of us, men learned and otherwise, have scarce a representative in Congress. Except for work among ex-soldiers, our Federal Health Service is starved; our feeble attempts to obtain a Cabinet officer fruitless. France, in her recent election, placed forty-one doctors in her legislative branch, which equalled the quota of the agriculturalists and nearly that of the industrials. England has won her Ministry of Health. We are beginning, when Iowa really assumes the burden of illness and cuts the corners to do it, and when Wisconsin is spreading clinical instruction through-

out the state. Midwifery can claim to have made a fair start, as we shall hear tonight. What gynecology should undertake in the way of preventive work we shall hope to hear from the author of the new textbook on industrial hygiene, Colonel Mock, whom many of you will remember as one of the most effective men on the Surgeon General's staff during the war.

From among the strictly technical social problems that are in our hands, a few instances may be given. Is there a simple method of preventing propagation among women who are idiots, epileptic, hopelessly insane, or incurably criminal? Does the cautery-tipped sound produce an effective stricture, passed into the uterus to the tubal opening? Is Cary's new test safe and sufficient, wherein, with the patient in the knee-chest posture, sterile fluid injected into the cavity of the uterus disappears if the tube is patent and not otherwise?

At the opposite pole from sterilization, with its enormous potentialities of betterment of the race, is artificial impregnation. It is a field almost unstudied. Dublin proposes to open the abdomen after a few months of sterile married life, and Boston and Brooklyn do so without trial of this simpler means. This procedure is an excellent instance of the need of collective experimentation, since no one man is likely to have a large experience.

Another of the distasteful subjects we naturally shirk is contraception. What serious study has ever been made bearing upon the harm or harmlessness of the variety of procedures, or concerning the failure or effectiveness of each? Who has or can acquire any considerable body of evidence on these matters but ourselves? What, indeed, is normal sex life? What constitutes excess or what is the penalty for repression in the married? Do we still have to hark back to Luther for an answer? It will take a few professional lifetimes of accredited histories to gather evidence to submit, but some time a start must be made.

SIMPLER LIVING

What about standards of living? There was a man named Tarnier, honored beyond most men in our craft. He lived and saw his patients in a modest apartment up three flights of stairs. There were a host of men of a nation whose chief genius gave the Society its motto, a nation possessing beyond others the infinite capacity for taking pains, and these teachers and students of the modest life put us to shame by their scientific product. In our land the doctor must be a failure who has not a car and clothes as good, and house and habits of the class of his best patients. Sessions for pure science must be housed in palatial hotels. A whole house for a doctor with offices busy for one work-hour in four is the custom in most of our towns. This is a formula which any business would condemn. Little wonder that the pace is such there is scant time for study or clinical travel. The younger man cannot voice the protest. It is for us of the senior group to voice—and act out—a protest against over-weaning costs of practice. There could hardly be a greater dis-service to science than to appear to give sanction to the dollar-gauge on achievement.

GYNECOLOGIC CENTERS

What should one do with a collection of charts and slides and models accumulated through nearly forty years? A college needs only teaching outfit; that is, a relatively small amount of carefully selected illustrative material. Books are welcome to the medical libraries, which wisely distribute them. So also with the great copperplate editions. But the fine original drawings that publishers of medical magazines and books accumulate, are in time destroyed. Many of them are masterpieces of technic. In the home of the College of Surgeons here in Chicago or in the museums which Washington, Boston and Philadelphia possess, the casts and specimens may be taken. But the libraries and museums have not sensed the need or else have not the room for a slide library or a chart-and-drawing collection. Think what a saving it would be if you or I, in expounding our epoch-making discoveries, could get, as introductions to our subject, all the best standard pictures for the asking. Furthermore, in each large city there should be a studio attachment of a simple kind to library or museum where the medical author and his artist can work.

SEX INSTRUCTION

Parts of sex instruction belong to us, and we may well be chief counsellors in the determination of the details of this curriculum. The subject comes forward this evening, but I may here merely say that I do not see how the man in our line can escape the responsibility for imparting certain portions of this teaching himself, or else seeing to it that the family doctor or visiting nurse undertakes these. To the mother, in order to forestall the gonorrheal vulvitis of infants, book teaching without demonstration is insufficient. This also applies to the prevention of vulvar irritation and watchfulness lest the normal degree of autoeroticism go beyond bounds. To the young man and young woman, separately, a few days before marriage, who shall speak, and cover by instruction and warning those things concerning which our patients make this bitter and merited reproach, "Why did not someone tell us in time?"

Furthermore, an authorative pronouncement is overdue bearing on the necessity of certain routine pelvic examinations. Concerning the prevention and early detection of cancer by this means, we are actively educating the profession. We may advocate, not only routine pelvic examination before marriage, but a physician's certificate, and in time will require that the certifier have specific qualifications to have his paper honored. The premarital test can include fitness for maternity. Next would come routine prevention care by examination of one's patients in the second month of pregnancy to forestall a large number of needless miscarriages. Before admission to certain forms of heavy work certificates based on examinations will one day be an accepted practice. All these are samples of gynecology at work in prevention instead of patching. We should be the counsellors to whom social workers turn, but we need to take thought in order to get ready with the answers.

JOURNAL

As an example of the type of concrete and visible activity on the part of the Society, there may be instanced the initiation and responsibility for the editorial conduct of a technical journal. With a just proportion of the members of the editorial board named by this Society, its standing is sure to be high, and free from detrimental influence.

TEXTBOOK

As the Society may wisely have under its auspices a periodical, so it may foster the publication in book form, of approved principles and practice and the necessary basic facts on which these are built, provided an adequate textbook is not produced or producible under the ordinary conditions of free competition. The Army set us the example. It is held by Holden that a satisfactory student's manual, containing all that it is desirable to teach him, should be very fully illustrated and have many cuts in color. Such expenditure is declined by publishers because the sale may not extend far beyond the writer's own classes. A manual brought out and approved by several prominent teachers and known to have the endorsement of the Society will be remunerative, even though kept up to date.

CERTIFICATION OF SPECIALISTS

This Society in its yearly meetings is the great clearing house for ideas and the greatest annual stimulus most of us get withal. But if it is the desire of the membership to exert upon the practice of gynecology and obstetrics the widest possible influence, it will start now to lead in that certification of specialists which is bound to come. Our survey will demonstrate which institutions have outfit and output of such high grade that their graduates may be endorsed. Our standards will automatically test and our publicity of end results scale their workers, and though we err often in method or man, it is surely only by trial and open mindedness that we can effect any progress.

Such certification should have its time limit, and be renewable, let us say, every ten years. Even membership in this Society might be predicated on worthiness, not past, but present, with proof forthcoming every decade. What evidence has the State that my degree of thirty-eight years ago still keeps me fit in character and knowledge to practice medicine?

WOMEN IN OBSTETRICS AND GYNECOLOGY

"Men and their trades unions," says Macassey, "have always been singularly successful in staking out their claims to the most highly remunerative classes of work, around which they have erected impenetrable barriers against the entry of women." Though we older doctors have seen our hospital wards humanized by the exaltation of the woman nurse and giving her control, we have refused to give her any real opening to qualify for the inside of the barrier by declining free access to interneships and to assistant and associate positions. The limited exception is to be found in England,

where in Euston Road and the Royal Free Hospital for Women, Mrs. Scharlieb and her followers have made good in major work and, in the latter, alongside of men. If American women in their own medical colleges have suffered the inevitable consequences of inbreeding, the responsibility is with the men. "British women in industry," to quote Macassey again, "seemed temperamentally immune to the deadening influence of monotonous work, to which men are peculiarly susceptible. Paradoxically enough, when the work required constant alertness, a sure deft touch, delicacy of manipulation, in short, a combination of quick intelligence and manual dexterity, within a limited ambit, women were invariably superior to men." One would think Sir Lindley was describing qualifications for laboratory service, for dispensary or office treatment, for most of our operative technical craftsmanship. If his statement were applicable to American women, what a field for skilled associates, leaving us lords of reaction free for unlimited ambits. And what a chance to secure, for people recommended for operation, constant consideration as human beings. Think of tight lipped surgery with a tender heart.

By the way, are there no women who of right should be members of this Society?

TRAINING OF LEADERS

The shortage, commented upon last year, of men in our line who are of the caliber to head large services and to fill important teaching positions of the first and second ranks, is merely a striking instance of a condition that is general. The war mercilessly exposed the relative smallness of the number of men of the highest grade of ability in the profession. That it did the same for every other profession and business is scant comfort or excuse. The important thing to note is that some of the reasons are removable. Thus, as an example, only one surgeon in four of the fulltime surgeons in the Army reported himself as having a hospital appointment, and among the physicians only one in twenty-eight (28) had any hospital connection. This 14,000 first admitted to the army from civil practice ought to represent a cross section of the men in active practice in the profession (125,000). It should err on the favorable side, as there were 28 per cent rejections. After acceptance, the fulltime surgeons were grouped into classes. The grouping was based on first hand knowledge. Class A comprised men of the capacity of chiefs of large city clinics, able organizers and instructors; Class B, first assistants capable of assuming the chief's duties. Among the 14,000 there were as follows: Fulltime surgeons (self reported) 2884, or 20.7 per cent; fulltime surgeons (self reported) with hospital appointment 4.88 per cent; Class A surgeons 1.44 per cent; Class B surgeons 2.16 per cent; medical men with hospital connection 2.11 per cent.

This paucity of numbers to draw from must be remedied. This lack of leadership material must be met by developing to the utmost what training facilities we have until new resources take form. It cannot be a matter of indifference to this Society, therefore, and is a matter of no little concern to all workers in this department, to make sure that America's well-equipped teaching clinics are ably manned and working to full capacity at the job of

selecting and drilling the future teachers and investigators in gynecology and obstetrics. Our survey will demonstrate the facts. Publicity will do the rest.

PROGRAM

Here is a dream of a four year program for the Society, carrying out some of the visions of these wandering conceits. To be realized, they need but men and money and the driving force of an idea. Also flexibility.

First year: New journal under the auspices of the Society. Propositions for and action on those standards immediately required, such as details of the business of a typical hospital meeting for the weekly or monthly staff review of casualties. Self report by chief clinics, and on large cities. Report on participation in sociologic program. Tentative nomenclature of operations.

Second year: Report on colleges and obstetric-gynecologic services in general. Report on women in gynecology and obstetrics. Index of obstetricians and gynecologists looking toward certification. Standard methods, histories, indexes submitted for criticism or endorsement. Student's manual. Nomenclature of diseases and injuries and operations. Participation in public health movements affecting women, with recommendations based on these.

Third year: Publication of surveys and standards. Publication of best type of each form of service, private and outpatient, large and small; hospital, big and little. Plan for education of general practitioners at their home towns in everyday gynecology. Yearly paper on social or industrial subjects.

Fourth year: Certification of specialists.

To me this seems an inspiring program, and with it, whether carried on by us as individuals or by the Society, whether driven swiftly or slowly, the security of our future would seem to be assured.

438 West One Hundred and Sixteenth Street.

A DEMONSTRATION OF CERTAIN TRANSITION STAGES FROM BENIGN TO MALIGNANT CONDITIONS IN THE OVARY, THE UTERUS, AND THE VULVA*

By T. Watts Eden, M.D., F.R.C.P., F.R.C.S. and

Gorden Ley, F.R.C.S., London, England

IT has been recognized for a long time that there is no sharp histologic line of demarcation between benign and malignant tissues: many neoplasms occur which histologists regard as suspicious, but not definitely malignant: many others are on the borderline. Some observers would designate them as benign, others as malignant.

The early clinical diagnosis of cancer is of overwhelming importance. Yet early diagnosis is very difficult, because the early stages of cancer are so often unattended by symptoms, or attended only by unimportant symptoms.

These difficulties have led observers to endeavor to define conditions which, while not definitely malignant, yet show such a tendency to end in malignancy that they may be regarded as half-way houses; such conditions if they can be detected, would be rightly called "precancerous."

This demonstration, as illustrated in the accompanying figures, is the outcome of an attempt to explore the intermediate conditions as they occur in the ovary, the uterus, and the vulva, and if possible to bring them in relation with recognizable clinical appearances. The positive results are perhaps meagre, but the subject is not an easy one, and much further work is required.

OVARY

There is no evidence that ovarian cancer begins in normal follicles, we can only trace its origin from the epithelium of cysts. Therefore the cyst is the first departure from the normal, the first step along the path which eventually may lead to cancer.

Pseudomucinous Cysts.—Let us consider first these common, and quite benign growths. Their original structure in the character of their epithelium is shown in Fig. 1. The loculi are simple, i. e., they show no ingrowths, the mode of proliferation of these cysts being of the exogenous or everting type. The single layer of the epithelium is furnished with uniformly small, round basal nuclei.

Fig. 2, from an undoubtedly benign cyst, shows loculi with budding of villous processes growing into the interior (endogenous or inverting form of proliferation). This gives the loculi a more complex character than in Fig. 1. Further the epithelium is proliferating, the nuclei are irregular in size, are irregularly placed in the cells, and tend to become central instead of basal in position.

*Presented by invitation at the Forty-fifth Annual Meeting of the American Gynecological Society, Chicago, May 25, 1920.

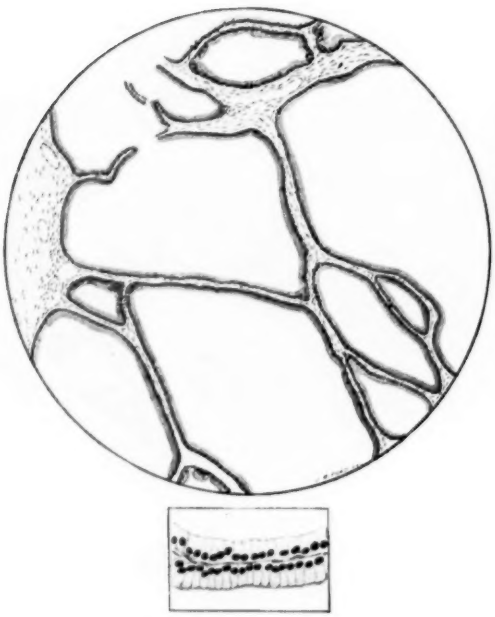

Fig. 1.—Section of pseudomucinous cyst of ovary (x 92) showing general structure (above), and character of epithelium (below) (x 334).

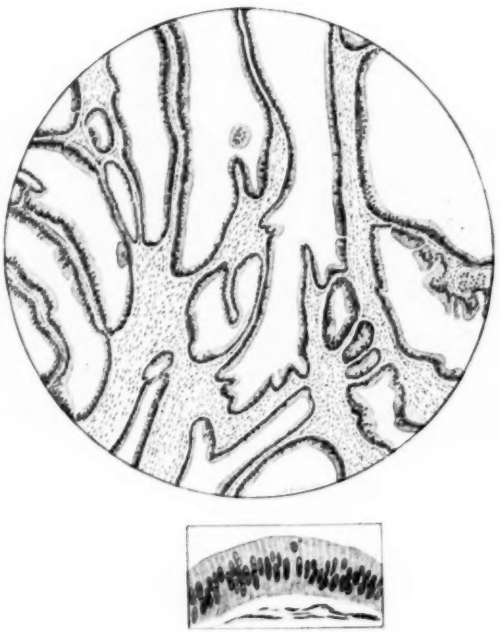

Fig. 2.—Section of benign ovarian cyst (x 92) showing endogenous or inverting form of proliferation. Character of epithelium with irregular nuclei (below) (x 334).

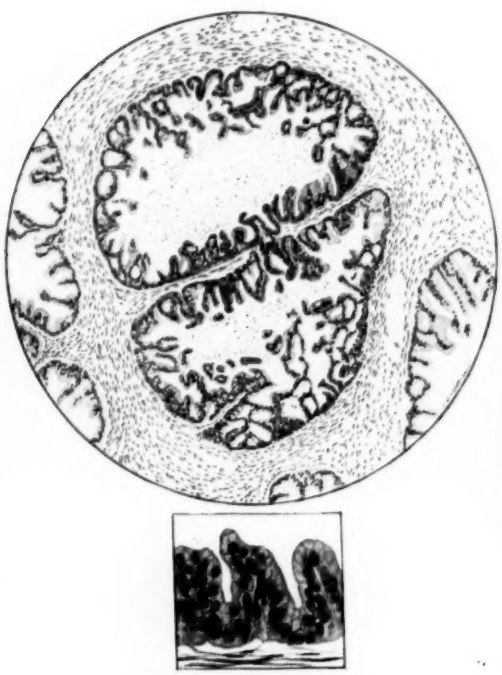

Fig. 3.—Section of ovarian cyst (x 92) clinically benign, showing villous formation of epithelium, tending to fill up the spaces.

Fig. 4.—Section of undoubted malignant ovarian neoplasm showing practically solid character with loculi disappearing (x 92).

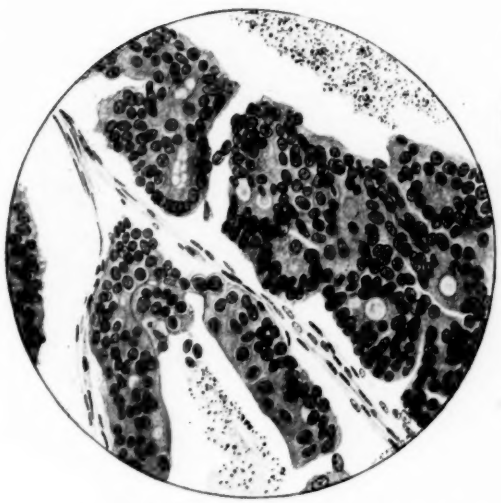

Fig. 5—Epithelium from section shown in Fig 4 under high power (x 334).

Fig. 6.—Section of ovarian cyst showing club-shaped intracystic papillæ (x 30). Cubical epithelium below (x 334).

Fig. 7.—Low power section of ovary, showing papillary cysts in hilum.

Fig. 8.—High power section through papillary mass of Fig. 7. Below is shown epithelium (x 334).

Fig. 3 is from a cyst which was clinically benign. The epithelium of the loculi shows abundant irregular, branching villous formations which tend to fill up the spaces. The epithelium shows imperfect cell definition, the outlines of the cell being difficult to trace, the nuclei are quite irregular in size and distribution. That rapid proliferation is occurring is shown by the accumulation of debris from detached cells in the center of the luculus. The stroma is normal and abundant and the outlines of the spaces are well defined.

In Figs. 4 and 5 we have an undoubtedly malignant growth; practically solid in general character. The regular locular formation has disappeared, but some irregular nuclei are still seen; the amount of stroma is scanty; the epithelium is a mass of solid, tubular, and papillary formations. These masses are multinucleated and vacuolated like plasmodium and they show no true cell divisions. There is much cellular debris in such spaces as remain. Fig. 5 shows the epithelium from the same tumor under a high power.

Fig. 9.—Section from an ovarian papillary carcinoma (x 92).

Fig. 10.—High power section of Fig. 9, showing scanty stroma.

Papilliferous Cysts.—The question whether these cysts really arise in Wolffian relics or in derivatives of the germ epithelium is immaterial for the present purpose.

Fig. 6, from a cyst of the ovary, shows the formation of solid club-shaped intra-cystic processes or papillae, having a well-formed fibrous tissue matrix. The epithelium is a single columnar or cubical layer, closely resembling that seen in the last section. The stroma of many of the papillæ is edematous.

Fig. 7 is a low-power section through an ovary showing small papillary cysts in the hilum, which are nearly filled up with abundant compound dendritic papillæ.

Fig. 8 shows a section under a high power through one of the papillary masses. The stroma is abundant and healthy. The epithelium shows great activity; there are many club-shaped and irregular, epithelial processes, due to sporadic proliferation. The cell-layer is irregularly multiple, and the nuclei are irregular in size, shape, and distribution.

Fig. 9 is from a postmortem specimen of a case which was undoubtedly a papillary cancer. The stroma is scanty, the papillar arrangement is preserved, but has become quite irregular. The papillæ are covered with many layers of cells cubical and polygonal in shape, and of a variable size, the nuclei irregular in size, shape, and distribution.

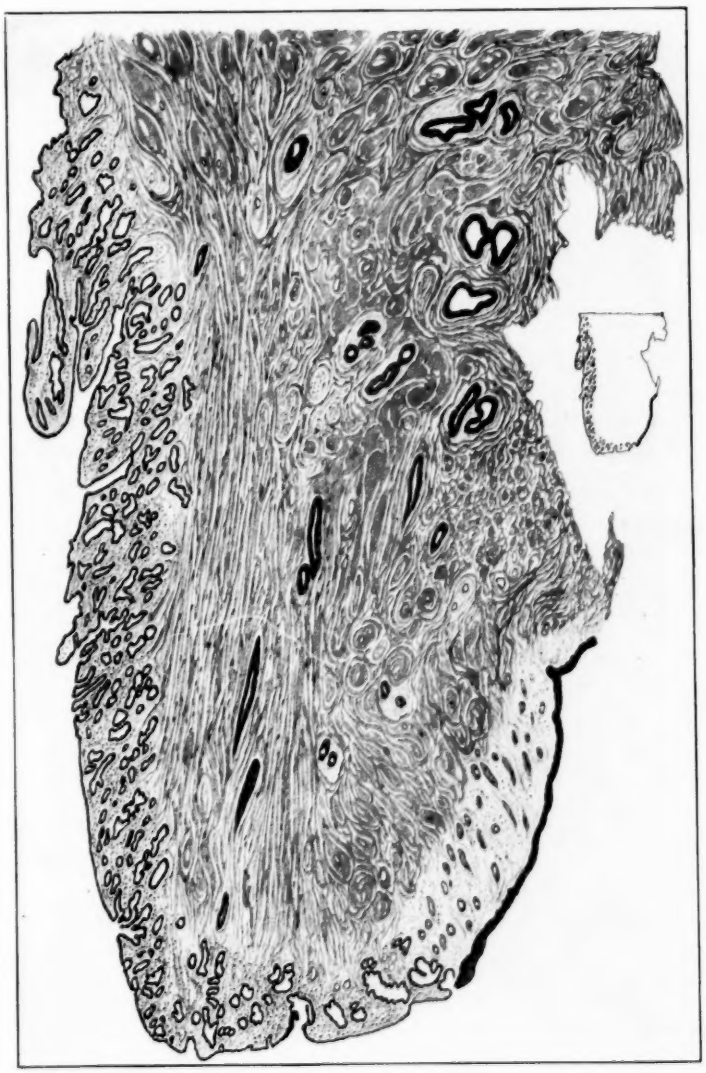

Fig. 11.—Section through lip of cervix showing erosion. (From Eden and Lockyer, *Gynecology.*)

Fig. 10, a high power picture of the former shows well the scantiness of the stroma and the variability in type of the epithelial cells in their nuclei.

These figures show that in both the common types of ovarian cyst gradual traumatic stages can be demonstrated from innocence to malignancy, so that it is difficult to define any dividing line between them. As it is our rule to remove all ovarian neoplasms as soon as they are recognized, the detection of pre-

cancerous condition is mainly of interest from the histologic standpoint. We do not know what is the cause of the formation of cystic tumors in the ovary.

In the remaining sections we shall be chiefly concerned with the effect upon the tissues of chronic irritation, beginning with the cervix.

CERVIX

Fig. 11 (taken from Eden and Lockyer's *Gynecology*) is a section through the wall of a cervix showing an "erosion" covering the whole of the free edge of the cervical lip. The mucous membrane lining the cervical canal is very thick and abundantly supplied with branching glands; many of these open by wide mouths upon the surface and others show dilatation of their deeper parts. The indentity of the mucous membrane lining the cervical canal and that covering the area of erosion is quite clear. The stratified epithelial covering of the outer wall of the portio vaginalis is normal; note that its deep surface forms a slightly wavy line, the intrapapillary processes being only slightly marked. Where it joins the edge of the "erosion," the stratified epithelial layer is a little undermined by the wide mouth of a gland.

In Fig. 12 the stratified epithelial layer is fairly normal. Beneath it is a deep zone of intense round-celled infiltration; in this zone are also seen a dilated gland lined with columnar epithelium, showing the two changes in combination, glandular epithelial activity, with early inflammatory changes.

In Fig. 13 is shown to the right, normal stratified epithelium gradually tailing off to the left to end in a denuded surface. Intense leucocyte infiltration of stroma under the denuded surface and around many of the glands. Stroma contains actively proliferating glands, some dilated, others not; the type of proliferation is the inverting or papillary type in parts, in others the ordinary everting type.

In Fig. 14 the epithelium is a single layer and the cells are of normal type. The condition is therefore benign, but it is probably the forerunner of tubular cancer of the cervix.

Fig. 15 shows the edge of the cervical lip. The surface epithelium is greatly thickened and actively proliferating; there are broad, branching, intrapapillary processes dipping down into the stroma, some of which are seen in section as islets. Others are penetrating the gland spaces and are possibly solid processes traveling along the gland duct (this is suggested by the corkscrew arrangement). There is slight leucocytic infiltration. The condition cannot be definitely classed as either benign or carcinomatous but would have terminated as cancer, i. e., it is precancerous.

In Fig. 16 is shown to the left a normal stratified squamous epithelium. Beyond this the epithelial surface is broken by the mouths of glandular spaces; these are lined in parts by stratified squamous epithelium, in parts by typical columnar epithelium. The former show short papillary downgrowths; beyond the mouth of the second gland the surface is again seen to be formed by an atypical stratified squamous epithelium, with short papillary down-growths. Beyond this is a normal gland. There is marked leucocytic infiltration in places. In the stroma is a dilated gland tube which shows inverting papillary processes,

one of which is composed of stratified squamous epithelium. Where the stratified epithelium is in relation to the gland tubes its type of proliferation is quite atypical; this is probably a metaplastic change, the columnar epithelium becoming stratified squamous.

In Fig. 17 the section is through the edge of an erosion which has been attacked by cancer. The left half shows on the surface a thickened stratified

Fig. 12.—Section from cervix showing inflammatory reaction. (From Eden and Lockyer, *Gynecology*.)

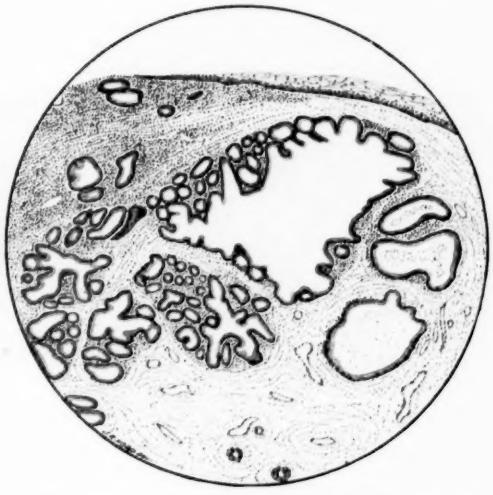

Fig. 13.—Section from cervix showing marked glandular proliferation (x 30).

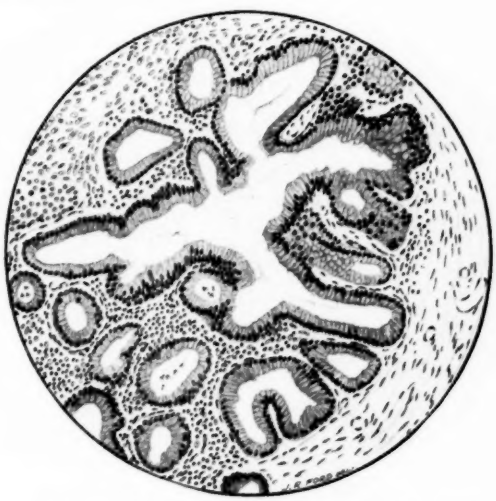

Fig. 14.—High power of Fig. 13, showing normal cells in a single layer, although benign, is probably forerunner of tubular cancer.

Fig. 15.—Section from edge of cervix, showing marked proliferation of the squamous epithelium.

layer, with thick epithelial down-growths, some appearing as islands surrounded by stroma. The right half is recognizable as the seat of an erosion from the general contour of the surface and the wide compound gland-tubules which open on it beneath this part of the surface is a dilated gland space. Both the surface and the gland spaces are covered and lined with greatly thickened epithelium

of stratified type, which also has invaded the stroma beyond the limit of the section. The change appears to have affected both types of epithelium quite equally. In benign conditions we have seen that the two types of epithelium readily replace one another, e. g., in the formation of an erosion and in its subsequent healing. The same transition is seen in cancer affecting the cervix.

Fig. 16.—Section from edge of erosion attacked by cancer.

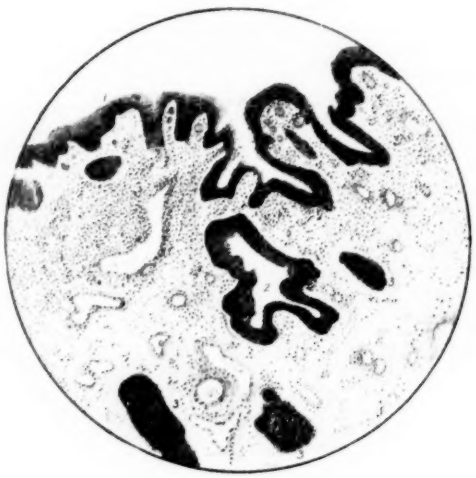

Fig. 17.—Section showing a squamous and polygonal-celled cancer of vaginal cervix and canal. (From Eden and Lockyer, *Gynecology.*)

Fig 18.—Section of a polypoid mucosal adenoma of cervix.

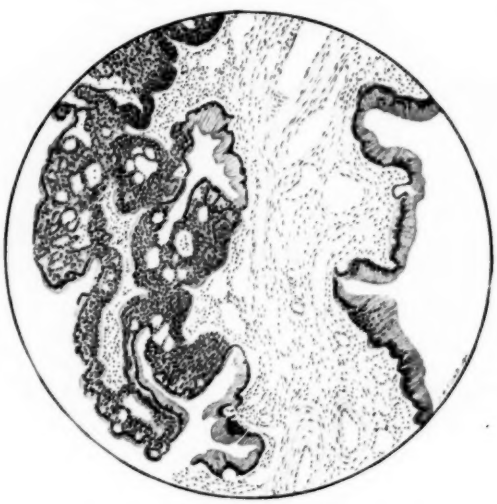

Fig. 19.—High power section of Fig. 18 showing conversion of high columnar into stratified squamous epithelium.

Cervical Polypus with Early Cancer.—Fig. 18 is a section showing a part of a polypoid mucosal adenoma of the cervix; on one surface the typical columnar has been largely replaced by a stratified squamous epithelium. The latter is actively proliferating, showing surface papillary projections and down-growths into the stroma. The conversion of high columnar epithelium into stratified

squamous is well shown in the high power slide (Fig. 19) where the stratified squamous processes appear to originate from the basement membrane of the columnar epithelium covering the surface and lining the gland.

CORPUS UTERI

In Fig. 20 is shown the menstrual gland with budding of glandular epithelium, swelling of stroma cells, and distention of capillaries.

Fig. 21 shows an example of benign cystic glandular hyperplasia. The gland tubes are irregularly dilated, tortuous, with considerable intratubular papillary proliferation. Cells are of normal type nuclei being regularly arranged.

Fig. 22 is from curettings obtained from a case of benign glandular hyperplasia. Gland tubules are numerous, little dilated, and in places somewhat closely packed; atypical epithelial proliferation is very free. There are short, irregular papillary projections into many of the gland tubes.

Fig. 20.—Showing menstrual gland from corpus uteri. (From Eden and Lockyer, *Gynecology*.)

Fig. 21.—Showing benign cystic glandular hyperplasia of corpus uteri (x 60.)

Fig. 23 is a section from the edge of an atypical benign sessile localized adenoma of the corpus uteri. To the right is seen a normal endometrium with simple straight tubules. To the left are seen numerous closely packed tubules, varying greatly in shape; the columnar cells lining them have their nuclei at varying levels in many places and the cells are in more than one layer. This point is best seen in the high power. The upper layers are from the normal endometrium, the lower layers from the adenoma. In a curetting, very probably, the pathologist would return this as carcinoma; it is no doubt precancerous.

The next section (Fig. 24) is a curetting taken from a relatively normal (i. e., benign) area of a tubular columnar-celled cancer. The uterus was removed and a diffuse cancer of the endometrium was found; the area shown is similar to that in the last slide. Yet this was from the general point of view an undoubted cancer, while the former from the general point of view was undoubtedly benign.

In Fig. 25 is shown the edge of a papillary cancer of the endometrium. To the left is seen the normal postclimacteric endometrium; the mucosa is narrow, the gland tubes are scanty, and the stroma fibrous. To the right are seen irregular closely packed spaces and papillary processes; the former are in places lined by a columnar epithelium with markedly irregularly arranged nuclei, in places filled by solid masses of polygenol cells; the latter are covered by cells of similar type often in many layers. The scanty stroma shows leucocytic infiltration.

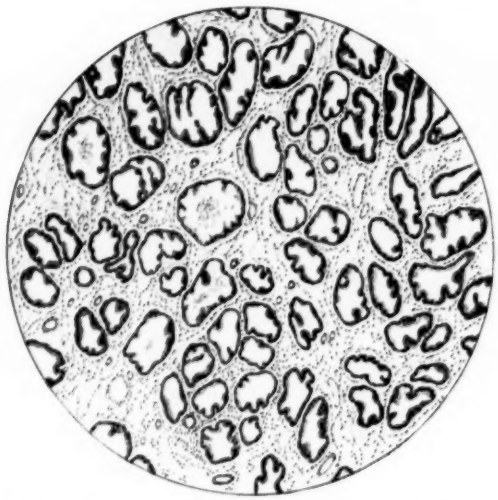

Fig. 22.—Showing benign glandular hyperplasia (x 65).

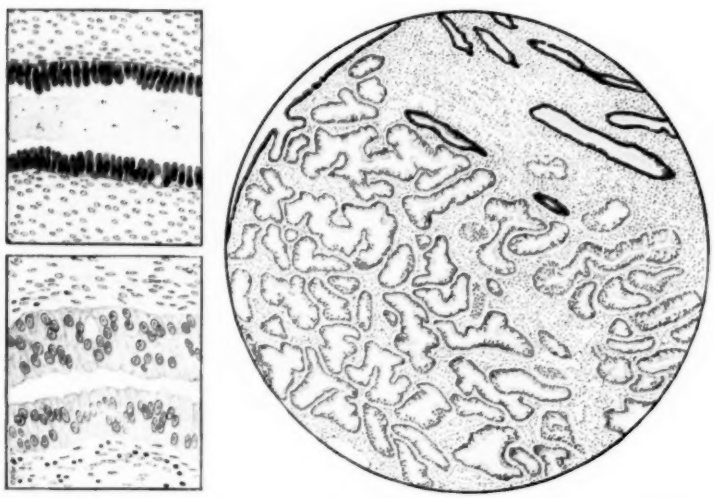

Fig. 23.—Showing atypical benign adenoma of the corpus (x 60) (above), and character of the epithelium (below) (x 240).

In Fig. 26 is a section from a case of leucoplacia. The horny layer is moderately thickened and stratified; the subjacent epidermal layer is greatly thickened, showing broad, irregular, deep interpapillary processes, with simple branchings or divisions; beneath the epithelial layer the dermis shows marked leucocyte infiltration. By fusion of epithelial branches islands of the dermis have been isolated. Epithelial proliferation and inflammatory changes are thus seen together.

Fig. 27 is a section from a case of pruritis with chronic vulvitis showing a similar condition under a high power. The horny layer is not well marked. The epidermal layer has the general characters seen in the last section. There is a very acute leucocytic infiltration around the bases of the interpapillary epithelial processes (high power), indicating marked irritation.

Fig. 24.—Section of columnar celled cancer (x 80).

Fig. 25.—Section showing edge of a papillary cancer of the endometrium (x 60).

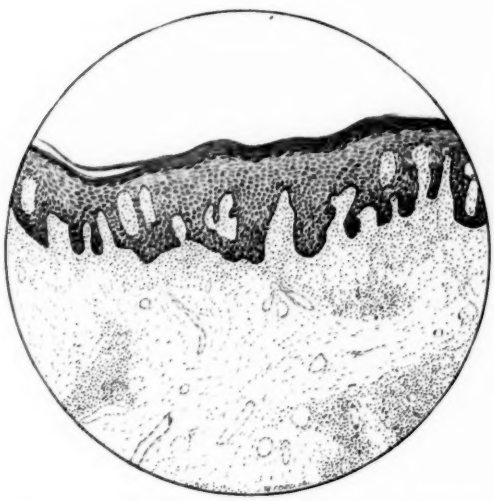

Fig. 26.—Section from a case of leucoplacia showing epithelial proliferation and inflammatory changes.

Fig. 27.—Section from a case of pruritus with chronic vulvitis (x 65). (From Eden and Lockyer, *Gynecology*).

In Fig. 28 the interpapillary epithelial processes are deep and irregular. The horny layer is absent and over a large area it has been replaced by inflammatory exudate, the whole epithelial layer is comparatively thin, but the ulceration has not penetrated through it completely. The dermis is extremely vascular and shows leucocytic infiltration. There is no approach to malignancy as a result of the ulceration.

Fig. 29 is a low power section through the cancerous area showing the leucoplacic changes at the extreme margin. Solid masses of epithelium have grown down for a considerable distance into the dermis; they are quite irregular in shape and are evidently fusing with one another. The surface is broken on the summit of the cancerous area (ulceration). The high power is through the edge of the cancer. On the left are the leucoplacic changes seen in the previous

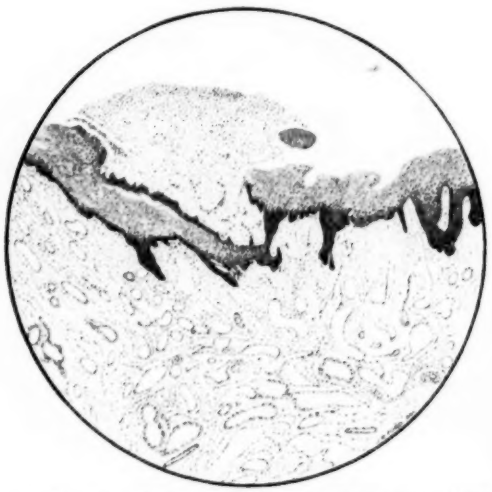

Fig. 28.—Section showing ulcerated area from leucoplacia, but with no approach to malignancy.

Fig. 29.—Low power section through a cancerous area, showing leucoplacic changes at the margin. High power below (x 592) through edge of cancer.

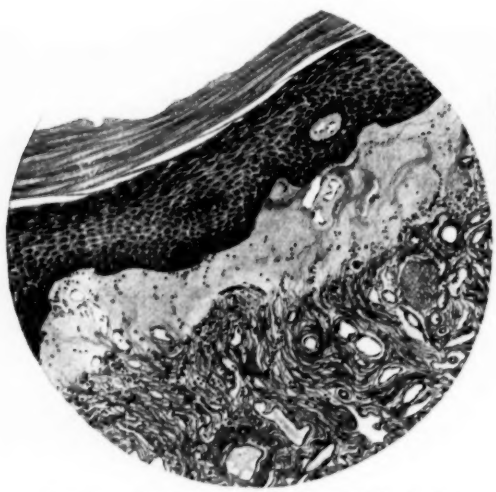

Fig. 30.—Showing a healing leucoplacia (x 70).

section, down the center of the field is a hair follicle. To the right are epithelial processes definitely cancerous, invading the dermis and subdermal tissues to a considerable depth. In this case an enlarged inguinal gland was removed which was definitely cancerous, thus confirming the diagnosis.

Fig. 30 is taken from a case of healing leucoplacia.

CONCLUSIONS

1. It must be admitted that, speaking generally, the transition from benign to malignant histological characters is so gradual that it is very difficult to define a stage which can usefully be called "precancerous."

2. In the case of the ovary the detection of precancerous conditions is not of great practical importance if the rule is observed that all ovarian neoplasms should be removed as soon as they are detected. There is clear evidence that cancer occurs as a rule in an ovarian cyst, not in an unaltered ovary.

3. In the case of the uterine body, we hold that the condition shown in Fig. 23 is precancerous and such appearances, or any closely resembling them, should form an indication for panhysterectomy. The outstanding feature is the abundant atypical proliferation of gland tubules, and of the epithelium lining these tubules.

4. In the case of the cervix there is clear evidence that an erosion may heal, the columnar epithelium and glands being replaced by stratified squamous epithelium; this may occur either under treatment or spontaneously.

In other cases the erosion becomes the seat of a cancer and so far as we have seen, this is of the squamous-celled type. It will probably become in time established that it is the erosion which is accompanied by deep cervical laceration, by thickening an deversion of the lips of the cervix, and histologically by abundant round-celled infiltrations, which is most liable to become cancerous. The form which occurs in nulliparous women is probably more benign. We therefore think that all standing cervical lacerations with thickened and everted erosions should be regarded as precancerous, and should be dealt with by free local excision. A radical operation is not called for in this condition.

5. Cervical adenomatous polypi may become also malignant, but we have been unable, in the case of these very common neoplasms, to find anything that could be called a precancerous stage; early removal is the universal practice and the point is therefore not of great clinical importance.

6. With regard to the vulva, we hold that chronic vulvitis is, generally speaking, not precancerous. Leucoplacia, like cervical erosion, may heal under treatment, or may go on to the formation of squamous-celled cancer. A leucoplacia which is accompanied by an enlargement of the inguinal glands is precancerous and should be treated by free excision of vulva and the glands.

We have seen three cases of glycosuric vulvitis which developed cancer and we suggest that this form of vulvitis may prove to be specially prone to be followed by cancer; further evidence is in the meantime required on this point.

We have found no evidence that gonorrheal warts show the least tendency to become malignant.

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THE INDUCTION OF LABOR AT TERM*

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FROM the time of Van Deventer (1701) to the present, our best obstetric thought has been devoted to the study of contracted pelves and the dangers and difficulties which these anomalies develop in the course of pregnancy and labor. This work has been pursued with an engrossing interest and has brought about additions and technical accessions to our knowledge of wide and far reaching importance, so essential and vital indeed as to constitute the very foundation of obstetric science.

Meanwhile but little attention, comparatively, has been paid to the corresponding problem which concerns the large, the mature and the postmature child. It is obvious that the large child, whether mature or postmature, must meet with obstruction in a normal pelvis and produce just as serious a complication of labor as the generally contracted pelvis with which it competes for an evil preeminence.

In spite of narrower interest, however, this aspect of the subject has been approached from various angles and one man after another has investigated and solved with reasonable accuracy several of its phases. These results are quite practical and may be summarized for specific application.

The duration of pregnancy is an important feature which has been reported upon by Ahlfeld, Casalis, Issmer, v. Winckel and others. Their work leads them to conclude that the average period for human gestation should be set at from 270 to 280 days with an allowance above and below these figures of about thirty days to provide for the uncertainty as to the incidence of fertilization and the accidental element in the onset of labor. They generally accept 275 days as an approximation to the actual elapsed time between conception and fetal maturity. This period is remarkably exact if we consider our entire ignorance of the date of fertilization and of the phenomena that determine the onset of labor. The end of the pregnancy is conveniently recognized by the word "term," which is not only too broad but wholly noncommittal.

It is a matter of common knowledge that a pregnancy may either be abbreviated or prolonged beyond the time set, but the prolongation of pregnancy is the less frequent. This phase of the question has been worked out by Parvin, who found that from 6 to 8 per cent of all pregnancies are more or less prolonged.

v. Winckel again has examined the effect of this delay in the onset of labor upon the fetus *in utero* and concluded that the phenomenon was regularly associated with large babies. He became convinced also that of all babies weighing more than eight and one-half pounds, 71.8 per cent were postmature. In other words, whatever the period of gestation, the time of fetal maturity had been overstepped.

*Read (by invitation) at the Forty-fifth Annual Meeting of The American Gynecological Society, Chicago, May 24-26, 1920.

It is evident that neither the menstrual history nor the known date of coitus does, or can, give us more than an approximation to the actual date of labor. Some other basis must be found from which to calculate the duration of pregnancy, or at all events, the proper time for the onset of labor. It seems more logical, as it certainly is more promising to determine the time for the normal onset of labor, not by the days or weeks spent by the egg in the uterus but by the effect of that incubation on the fetus. The purpose of pregnancy is the production of fetal maturity. When the child is mature, the normal end of intrauterine life has been accomplished.

What constitutes maturity? If we can establish the signs of this phenomenon and have the means of recognizing them in the uterus, it will be possible to say at a given time that the mission of gestation has been fulfilled.

Theoretically the child should be mature when the embryonic type of cell has been transformed into the mature type, but this is not so. Ballentyne, with good reason, bases the attainment of maturity on the physiologic perfection of the organs. He makes the point that as soon as the organs are prepared to functionate satisfactorily in an extrauterine environment, the fetus is mature.

This anatomic and physiologic perfection, however, must needs be secured by such a residence in the uterus as permits the child to achieve a constant and rapid growth, not only of the organs, but also of the structure and the connective tissue. This growth and development persists until not only the organs but certain more or less stable general characteristics have been formed and fixed.

The fetus now presents that physical appearance of wholesome sturdiness which is so quickly recognized as maturity by the expert, but is so difficult to describe. The child shows also a definite capability for an extrauterine existence, and we may therefore tentatively accept Ballentyne's definition of maturity as "that state or degree of fetal development which enables a child to surmount the perils and aggressions of extrauterine life easily."

Meanwhile, the child has acquired some rather definite characters which are authoritatively recognized as the gross evidences and criteria of maturity. The most important of these are the length and size of the fetus, and the fetal head diameters. On these phases of the problem much time and thought have been expended. The results of this work have been given in considerable detail elsewhere, but for the sake of completeness, it is desirable to give a summary.

The length, logically, should be regarded as the most important sign of fetal maturity. Fortunately the limits of this criterion have been widely discussed and rather generally accepted. Ballentyne, Hirst, Hecker, Webster, Edgar, Dorland, Cragin, Eden, Peterson, Ahfeld, Stumpf, Williams and Issmer agree that the length of a mature child should be 50 cm. to 51 cm. All admit the possibilities of 48 cm. and some extend the upper boundary to 54 cm. Our own series gave measurements that varied from 48 to 53 cm. with an average of 50.2 cm. All the babies were mature according to the definition. Babies of less than 48 cm., except twins, are not ordinarily mature, and babies cannot exceed 53 cm. in length without, in our opinion, entering the postmature class.

The size is next in importance. Without tiring you by a repetition of familiar figures, let us recall briefly that Hirst, Dorland, Edgar, Webster, Eden,

Goenner, Cragin, Jewett, Stumpf, Williams, Ahlfeld and MacDonald place the weight of a mature child at from 6 to 7½ pounds with a low limit of 5½ pounds and no mention of an upper boundary. Only by implication do we learn from this silence that babies that have long been mature must of necessity be postmature. The designation *postmature* is not common in literature.

The diameters of the fetal head are the next point that concerns us. v. Winckel states that the average occipitofrontal diameter in maturity is 12 cm. and the biparietal 9.25 cm. Our own series gave the occipitofrontal in mature babies as ranging from 10 cm. to 12 cm. and the biparietal from 8.5 cm. to 10 cm. In our opinion all babies that possess the above described characteristics of length, weight and fetal head diameters within the limits set are mature and those which register below or above these boundaries are, ordinarily, immature or postmature, respectively.

With these various measurements in mind, it is now possible to complete our definition and assert that maturity is that state or degree of development wherein the fetus is enabled to surmount the perils and aggressions of extrauterine life easily and that such development is associated with a fetal length of 48 to 53 cm.; a weight varying from five and one-half to possibly nine pounds and heads that measure from 10 to 12 cm. in the occipitofrontal and from 8.5 to 10 cm. in the biparietal diameters.

By this definition, if we use the word "term" at all, we should use it to signify that the fetus has attained physical maturity instead of to indicate that the uncertain end of an indefinite time is approximately due. Moreover it does not seem superfluous to reiterate that when the fetus falls within the scope of this definition, the end of profitable intrauterine life has been accomplished and a continuance thereof can only be an indifference, if not an invocation, to all those dangers, morbidities and mortalities which are so familiar to us in cases of contracted pelvis and therefore of the overlarge child.

It is not a catastrophe, ordinarily, for labor to occur a couple of weeks before the maturity of the child as we have defined it, but there is a steadily increasing danger to mother and child for every additional week of postmaturity. The child continues to develop at the rate of from 0.5 cm. to 1.0 cm. in length, and from one-half pound to one pound in weight each week and this increase in size necessarily intensifies the obstetric problem and by raising the chances of serious operative complications adds materially to the peril.

The intrauterine child goes to maturity just as, later, the delivered baby ripens into an adult. In both cases, where maturity is attained, if the nutritional intake exceeds the expenditure, the tissues will store up fat and become infiltrated with fluid. The abundance of this fat is not a sign of maturity, but of postmaturity. These accessions of fat, moreover, are as worthless to the infant as to the adult. An excess of fat and fluid in the tissues of the baby hampers rather than facilitates organic action and furthermore, both fat and fluid are lost rapidly after birth. As we have shown elsewhere, babies that average 7 pounds lose 8½ ounces in the two and one-half days following birth and then begin to gain, while babies that average 9⅒ pounds lose 14½ ounces in about the same time before the gain begins. The increase in bulk, therefore, has served merely to complicate labor without any corresponding advantage to the child.

A consideration of the data thus reviewed has led us to believe that it is timely and desirable to apply to all cases of fetal maturity the principles of management that we sometimes employ in contractions of the pelvis. In other words, when the child becomes demonstrably mature, labor should be induced at the first convenient opportunity. In pelvic contraction, to be sure, the anomaly becomes apparent as soon as the appropriate tests are applied, while the presence of a mature child has rarely been regarded hitherto as even a potential peril. Nevertheless where we induce labor for pelvic contraction, we act, if we are wise, before the crisis appears even though the child be submature, so why hesitate to avert a similar complication when maturity is present.

The diagnosis of pelvic contraction after two centuries of education has become a more or less successful routine, but the diagnosis of fetal maturity which is far easier is not so common. The methods for determining maturity have been before the profession for some years, but they have not as yet received the stamp of approval and current usage.

The technic of these methods is doubtless familiar, at all events descriptions are readily available in the literature and need not be dwelt upon here. It is desirable, however, to report our own experience with them.

The length is ascertained by Ahlfeld's maneuver. In our series the postpartum figures tallied exactly with the antepartum estimate in 37 per cent. The variation was less than 0.5 cm. in 24 per cent and less than 1.5 cm. in 29 per cent of the cases.

The weight and size of the child are estimated by the method brought out by McDonald some ten years ago. He assumes that when the uterus contains a mature fetus weighing 7⅓ pounds (3300 grams) the fundus will be 35 cm. above the symphysis. He also estimates that the weight of the child will be increased or diminished by 200 grams for each variation of 1 cm. in the height of the fundus. So far as concerns the maturity of the child we found this method practical and safe, but the estimates of weight were not so reliable, although they did not pass the limits of security.

The problem of the fetal head diameters was one of absorbing interest. Only two, of course, are possible, the occipitofrontal and the biparietal, but fortunately these are the ones most needed. They are obtained by Perret's maneuver. Thus the occipitofrontal is measured as it lies more or less transversely across the inlet without making any allowance for the thickness of the abdominal walls. From this result the biparietal is estimated by a system of deductions according to a scale suggested by Perret and elaborated by McDonald. If the occipitofrontal measures 12 cm., for instance, 2.5 cm. is deducted to get the biparietal. From 11.5 cm. occipitofrontal we take 2.25 cm., from 11.25 we take 2 cm. and from 10.0 cm. we take 1.5 cm. to get the biparietal.

In our series the postpartum measurement of the occipitofrontal tallied exactly with the antepartum estimate in 40 per cent. The variation was 0.25 cm. or less, in 34 per cent, and within 0.5 cm. in 24 per cent and erred by 1 cm. in but 4 per cent of the cases.

The biparietals obtained from the above occipitofrontals were found postpartum to be exact in 36 per cent, within 0.25 cm. in 31.7 per cent within 0.5 cm. in 24 per cent, and to vary by 1 cm. in but 6.5 per cent of the cases.

It is obvious that these differences are too slight to affect the diagnosis appreciably. All the babies but one were mature. Our sole failure was due to the stupidity of an interne. No system or technic however can be entirely free from the possibility of abuse. These maneuvers are quickly learned, their application is easy and the results surprisingly accurate.

With the determination of the length and size of the child and the two principal diameters of the head, it would seem that the question of maturity is safely assured. The problem of the pelvis is admittedly a matter of technic and the sole remaining point is the induction of labor. Whether or not the maturity of the child be generally accepted as a satisfactory and legitimate basis for bringing on the labor, at all events, our ability to induce the delivery at will is an important addition to our obstetric armamentarium.

TECHNIC OF INDUCTION

The labor can be inaugurated by castor oil and quinine, by quinine and pituitrin or by the modified de Ribes bag (Voorhees bag). The castor oil and quinine acts in about two or three cases out of five, and most reliably when the patient is a little bit beyond her calculated date. The bag in our experience has been the most dependable and is therefore the favorite agent at our hospital.

The patient's bowels should receive attention the night before, and in the morning the external genitalia are given a careful obstetric preparation.

Assemble and sterilize by boiling for 20 minutes, a modified de Ribes bag (Voorhees) No. 4, a Simon speculum or vaginal retractor, a pair of long Pean forceps (dressing forceps will serve), two pairs of vulsellum forceps, two pairs of compression forceps, a Goodell dilator, a tenaculum forceps, a hand bulb syringe, or metal piston instrument holding six or eight ounces, and glass tubes or rubber connections for the bag and syringe. The bag and accessory apparatus must be tested for defects before using.

The patient, prepared as for delivery, is placed upon the operating table in exaggerated lithotomy position with the legs held by assistants or by stirrups.

The vagina is retracted, a smear made from the cervix and the mucous membrane wiped clean with pledgets. Anesthesia is rarely necessary even in primiparæ.

One lip of the cervix is seized by the vulsellum and brought down. If the bag has been properly prepared the os will admit it ordinarily without dilatation.

The bag is emptied of any residual air and the flat end pulled out. It is next rolled into a compact mass like a cigarette and seized by the Pean forceps so that the tips extend just to the largest diameter of the rolled bag. After anointing the bag with sterile glycerine it is passed into the cervix with the concavity of the forceps turned slightly toward the patient's left leg and as it enters the os the concavity is turned upward one quarter of a circle so that when the maneuver is completed the hollow of the forceps conforms to the flexure of the uterus. Release the lock of the Pean forceps. Connect the tube of the bag with the filling apparatus and force the sterile solution—lysol, boric acid or plain water—slowly into the bag. Do not overfill by force, or the bag will break.

Tension in the tube leading from the bag, or the feeling of resistance to the injection are signs of fullness to an experienced operator. If uncertain of the technic, a measured amount of fluid may be used. A piston syringe of tested size will also serve to inform the operator when the capacity of the bag—six ounces—has been reached. The Pean forceps are removed as soon as the bag is sufficiently filled to keep it from following the forceps in the withdrawal. Snap the compression forceps on the tube. Remove the vulsellum from the cervix and disconnect the syringe. Tie the tube firmly with tape. Remove the compression forceps. Place sterile pads on the vulva, one on either side of the tube. Remove the stirrups and pull the patient up.

The bag may, from either rapid or insufficient filling, slip out of the cervix before the uterine contractions begin. If this happens another bag should be inserted.

If the pains do not start within an hour, a weight of one or two pounds is attached by a tape to the protruding tube and passed over the foot of the bed or table. Usually in from five minutes to half an hour the contractions begin and the labor is under way. In a variable period, rarely more than four hours, the bag is expelled by strong pains, the dilatation is practically complete and the head follows the bag down into the pelvis, the membranes rupture and the second stage begins. From now on the case is managed according to general obstetric principles. The tedious, exhausting, and painful first stage has been materially shortened. The bag acts as a mechanical aid to cervical dilatation, a dynamic stimulant to the contractions, and preserves the membranes from injurious pressure until physiologic rupture occurs.

When the membranes have been accidentally ruptured by the insertion of the bag, no attempt should be made by pulling on the bag to mark the degree of advancement lest it come out and by suction bring down the cord. Also when the bag comes out after such an accidental rupture of the membranes, it is good practice to make an internal examination to discover the presence or absence of a prolapsed cord.

In the series of 200 cases hitherto reported we had 114 multiparæ and 86 primiparæ. The average duration of labor was seven hours and fifty-six minutes. The longest labor was 30 hours and was due largely to a tough, inelastic cervix. Two others were 28 hours long on account of cervical conditions. One had a mass of cicatricial tissue. The shortest labor was 55 minutes in a multipara and 60 minutes in primiparæ. The bag broke while being filled or shortly after insertion, nine times. A second bag was necessary in four cases. The membranes were ruptured by the insertion of the bag seven times—in one case intentionally on account of hydramnios. The bag was expelled in an average period of 3 hours and 20 minutes. The longest detention was 9 hours, the shortest 10 minutes. Two mothers died. One had a myocarditis associated with marginal insertion of the placenta. Her labor lasted only an hour and a half and she lost less blood than usual, but she died two hours after delivery. The other had pneumonia and died eight days after delivery.

In no case did the bag fail to initiate the pains or the woman to deliver. The average weight of the babies was 7.4 pounds. The smallest weighed 5 pounds, the largest 10 pounds. Twelve babies died. One child of a primipara

was born in asphyxia after sixteen and one-half hours of labor, was revived with difficulty, and died eight hours later. One child of a multipara, born blue after a labor lasting one and one half hours. Revived. Died suddenly thirty-six hours later. Two died on second and fourth days respectively of hemophilia. One died of toxemia on the seventh day. One was the child of a primipara with a small pelvis. The delivery was instrumental and the baby died two hours after birth. One died after a forceps delivery, the occiput being posterior. Two were stillborn. One died in the course of a rapidly progressing second stage half an hour after the heart tones had been strongly evident. The other with heart tones diminishing was delivered with forceps but did not survive. Two died from the prolapse of the cord, one being luetic. One was premature as previously explained and the death was inexcusable.

The following operations were done: Version and extraction four times for cases of placenta previa transverse presentation, prolapsed cord, and to expedite labor in a heart case.

Forceps were used 39 times: occipito-posterior position, 11; deep transverse arrest, 15; insufficiency of the powers, 4; to hasten labor, 5; and for instruction, 4.

There were two cases of prolapsed cord.

Our employment of forceps may seem excessive to those who are guided by the statistics from large clinics wherein the women delivered are mostly normal and of strong physique. It does not seem improbable, however, that those of our colleagues who have much to do with our overcivilized American women will find in his private practice a larger percentage of forceps cases than in his clinical work.

Three of our women developed temperatures. One was due to pneumonia, one to tuberculosis, and one to mastitis.

No dilatation was required in any case prior to the introduction of the bag. In but five cases was anesthesia employed for the insertion of the bag and then for nervousness rather than for pain.

To these cases we now add 24 others that gave the following results: Multiparæ, 15; primiparæ, 9. Average detention of bag in utero, 4 hours, 1 minute. Average duration of labor, 6 hours, 11 minutes (1 history imperfect). Twenty-two babies averaged 7 pounds, 1 ounce. *Operations*: Low forceps, 8 (deep transverse arrest 4; prolapsed cord, 1; heart case, 1; inertia 2). Version and extraction 1 for prolapsed arm. Pubiotomy 1, for delay of aftercoming head. There were no maternal deaths. One baby died from prolapse of the cord. Pains began in all cases in less than an hour.

It is a pleasure also to report 34 cases from the service of Dr. C. E. Boys, of Kalamazoo. Thirty of these were inductions at term. Average detention of the bag in utero was five and one-third hours. Average duration of labor eleven and one-fourth hours. Pains began promptly in all cases but one, where they failed entirely. *Operations*: Forceps 10; version and extraction, 1. Largest baby 9½ pounds. Smallest 5⅝ pounds. Prolapsed cord, 2. Maternal mortality, 0. Fetal mortality, 3, two from prolapsed cord and one from forceps injury. In seven cases the temperature ranged from 100° F. to 101° F. on the

third day. All were operative cases in which the rise could be traced to other causes than the induction.

It is also my privilege to report 270 cases of induction from the service of Drs. George Clark Mosher and Buford G. Hamilton of Kansas City. One hundred ninety of these were for labor at term and 80 for moderately contracted pelvis or toxemia. In 24 cases the bag was used to replace prematurely ruptured membranes. In all cases castor oil and quinine preceded the bag by fourteen hours or more, but in only eighteen was the bag rendered unnecessary. The average detention of the bag *in utero* was six hours and twenty-five minutes. The average duration of the labor in multiparæ, eight and one-half hours; in primiparæ, twelve hours. The shortest labor was one and one-fourth hours (mp). The longest was sixty hours and required a second bag. Six cases failed to deliver after the bag was expelled.

Maternal mortality 5, from nephritis, endocarditis, and other complications independent of the induction. Babies' mortality 3.8 per cent, which is well within the average.

Operations.—Total for all cases about 35 per cent (forceps and versions). Mosher states that this is about the average in their general work where the fetal heart tones are the principal guide for interference. The cord prolapsed in three cases, and in one apparently followed the descent of the bag.

All cases were checked by routine measurements before and after delivery. and the bacillus coli were found, but though the induction proceeded, we had no appreciable displacements of the presenting part, and no hemorrhages chargeable to the bag.

Certain objections have been urged against the use of the bag for the induction of labor. The most persistent of these is the menace of infection. In the early cases this fear was constantly before us and cultures and smears were made from the cervix in every instance. Streptococci, staphylococci, gonococci, and the bacillus coli were found, but though the induction proceeded, we had no postpartum infections. This result was quite puzzling until it occurred to us that the comparative shortness of the labors might be a factor in conserving the patient's immunity. It is our conviction now that prolonged labors, though nonoperative, are more disintegrating to the morale and more disastrous physically than extremely painful deliveries that are soon terminated. This opinion might be stretched enough possibly to cover the operative deliveries in which the trauma is not excessive. Lynch, Mosher, Boys, and others who have used the bag sufficiently agree thoroughly with the writer that the danger of infection may be disregarded if a good technic is possible.

It has been charged also that the introduction of the bag may disturb the presenting part and cause malpositions. This possibility, which always hung over us in the earlier days when the Carl Braun bag was commonly employed, and even later with the huge de Ribes affair, does not seem to be much in evidence at present when the small flat topped fabric bag is used. A further insurance against the accident inheres in the self-control of the operator who contents himself with leaving the bag in the cervix where it belongs in place of trying to carry it to the fundus. So while admitting its possible or occasional occurrence, as Brodhead has also noted, yet we must regard the dislocation of

the presenting part as extraordinarily rare. Moreover if the head is lifted by the introduction of the bag it must soon resume its original position when the bag descends. In our second series we kept careful records of the positions and could not find an appreciable variation from the usual averages.

Whether a disturbance of the head is sufficient to encourage a prolapse of the cord is still uncertain. Lynch maintains that a long cord is prerequisite to a prolapse. If it happens therefore that a long cord should coexist with a marked disturbance of the presenting part it is probable that the mechanical conditions are present which permit a prolapse.

It is further charged that the bag is unreliable and that in many instances the pains cease after the bag is expelled and the induction fails. Although we have not had this experience, yet Lynch reports eight, Mosher six, and Boys one. We must therefore accept this event as infrequent but certainly possible in the hands of unimpeachable technicians. However, the bag is not an intelligent instrument and it cannot be left to its own devices if good results are desired. It may break or come out, it may leak or faii to advance and so the process must be watched assiduously from the moment of the insertion of the bag. Occasionally it may be advantageous to swing the bag to one side or release it from the weight so that it may change position or shift its point of pressure. When the pains are weak or slow in starting, it sometimes happens that a gentle massage of the uterus for a half hour or so will aid in bringing on or strengthening the contractions. The most common cause of failure is found in the practice of some operators who introduce the bag and then leave the case with no one at all or only an inexperienced technician in charge.

"Meddlesome midwifery" is the alliterative imputation that is loosely and almost instinctively made against the induction of labor by those who wear congenially the trammels of tradition. Wright, of Toronto, answers this objection quite clearly. He says, "If we can perform this operation in such a way that it causes no danger, or at least very much less danger to the patient than the prolongation of the pregnancy involves, then we must conclude that such interference is not only justifiable but advisable."

In our own experience at Wesley Memorial Hospital, it has seemed at times as if the pains of an induced labor were not quite so strong as in cases not induced. Whether the average of weak pains is larger or smaller than in noninduced cases we cannot say. Drs. Lynch and Mosher have both noted a diminished uterine irritability, a lessened responsiveness to the bag in cases of toxemia. Nevertheless Mosher insists that the bag is the most successful method of treating these cases. He also considers it a valuable aid in reducing infant mortality. It is generally accepted at present that an induction should be done in a properly equipped hospital and it is presupposed that the operator is familiar with the ways of measuring the child *in utero*.

In conclusion it seems to us that the modified de Ribes bag is a safe and efficient agent for the induction of labor at "term" and when properly used is open to but few objections.

Furthermore we think our experience justifies us in stating that not only is no harm done, but a large element of potential danger is averted by inducing labor when the child is mature. By this judiciously timed procedure, the con-

tractions of labor may be inaugurated when the child is ripe, but yet small enough to pass the pelvic canal without extreme difficulty. The mother is spared from four to six hours of suffering as well as serious operative trauma and she rises from her confinement quickly and with unimpaired vitality.

A minor feature, but one not altogether unimportant, is the knowledge that the gestation will terminate on a definite date. This is both a mental and a financial relief to the patient and a marked convenience to the doctor. Neither he nor the patient is taken unawares, but with the same equanimity and confidence that attends any other nonaccidental surgical engagement, the contractions are started in the morning, the labor advances smoothly and easily for mother and babe and terminates generally in the afternoon or evening.

Under such conditions of election, it is easily possible and convenient for the physician and his skilled assistant to give the patient the time and attention which all labors require and relatively few receive.

Medical thought is tending ideally toward prophylaxis. We strive to foresee and prevent pathology rather than await its onset. This is prudent prolepsis.

We believe that by the simple and easily applied tests which we have discussed in this paper fetal maturity *in utero* may be diagnosed definitely and unmistakably. We believe that the induction of labor by the method described is a safe and efficacious procedure. We believe that by combining these measures the uncertain arrival of a large or postmature child will become, as it should, an exceedingly rare occurrence and that it will happen only as an inadvertence, or as a result of temporizing which, not unjustly, would be looked upon with dismay in other branches of surgery.

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THE PROPHYLACTIC FORCEPS OPERATION*

By Jos. B. DeLee, M.D., Chicago, Ill.

THE time is not yet ripe for a general recommendation of the procedure to be described in this paper. As obstetric specialists, we must lead the way in improvements of our art, for this is still capable of improvement. The public is demanding with a voice that becomes louder and more insistent each year, relief from the dangers of childbirth for the childbearing woman. As regards the pain, the rapid spread of the twilight sleep craze will show that the demand for "tokophobia" is spreading among women.

If we study our cases carefully the conclusion is inevitable that while we have decidedly improved the maternal mortality and morbidity and have reduced the fetal deaths somewhat, labor is still a painful and terrifying experience, still retains much morbidity that leaves permanent invalidism. The latter statement is also applicable to the child. Many efforts are being made to ease the travail of the woman and to better the lot of the infant. What follows is another such effort. Experience alone can decide whether it accomplishes its purpose.

The "prophylactic forceps operation" is the routine delivery of the child in head presentation when the head has come to rest on the pelvic floor, and the early removal of the placenta. Primiparous labors and those in which the condition of the soft parts approximates a first labor, are treated by this method, which really comprises more than the actual delivery of the child. It is a rounded technic for the conduct of the whole labor, with the defined purpose of relieving pain, supplementing and anticipating the efforts of Nature, reducing the hemorrhage, and preventing and repairing damage.

It is not a complete reversal of the watchful expectancy that is universally taught, but I cannot deny that it interferes much with Nature's process. Were not the results I have achieved so gratifying, I myself would call it meddlesome midwifery. For unskilled hands it is unjustifiable.

A typical case is treated as follows: As soon as the pains are well established and the cervix opened two to three centimeters, the parturient is given 1/6 grain of morphine and 1/200th of the scopolamine. After one hour 1/400 of scopolamine is given and in one or two hours occasionally a third dose of the same size. The room is darkened and suggestion used as much as possible to aid the medicines. This is really a modified twilight sleep and usually the cervix dilates and the head comes down on the perineum without the necessity of further drugs. Occasionally 15 grains of chloral and 40 grains of sodium bromide are given *per rectum* to aid the morphine, or gas and oxygen are administered by an expert. It is important to obtain complete *spontaneous* dilatation of the cervix, and the slower the better. The importance attached to this point, the natural dilatation of the cervix and the slow retraction of the pericervical connective tissues, cannot be exaggerated. We are unable to imitate this by art.

*Read at the Forty-fifth Annual Meeting of The American Gynecological Society, Chicago, May 24-26, 1920.

When the head has passed the cervix and rests between the pillars of the levator ani and has begun, just begun, to part them and to stretch the fascia between them—a matter that is easily determined by rectal examinations, the patient is put to sleep with ether, and a typical perineotomy (soon to be described) is performed. Under the minutest possible control of the fetal heart tones—either the operator or an assistant listening every minute, with the head stethoscope—the forceps are applied and delivery accomplished. This is usually surprisingly easy. As soon as the child's head is born, 1 c.c. of Burroughs and Wellcome's Pituglandol is injected into the deltoid muscle. A nurse stands ready with 1 c.c. of aseptic ergot and this is injected into the outer thigh muscles as soon as the placenta is visible in the vulva. If there is hemorrhage, the placenta is removed at once, if not, we wait five to ten minutes. The operator either changes his gloves or disinfects them with antiseptics, and if the placenta is not already visible in the vulva, inserts the left hand into the vagina or the lower uterine segment, palm up, while with the outside hand the hard (pituitrin) uterus is pushed down on the already descended placenta. The placenta slides down the hand like a heel slides along a shoehorn. We call this method of expression of the placenta the "shoehorn maneuver," and it is the rare exception that the placental delivery needs more help than light pressure on the contracted uterus from above. Should there be any undue bleeding, another ampoule of pituitrin is injected directly into the uterine muscle through the abdominal wall. Uterine tamponade is almost never needed.

The woman is now given ¼ grain of morphine and gr. 1/200 of scopolamine to reduce the amount of ether required for the repair work, to prolong the narcosis for many hours postpartum, and to abolish the memory of the labor as much as possible.

It is surprising how bloodless the operative field, especially the cervix, has become. The cervix is pulled down with specially constructed ring forceps and all tears immediately repaired. I have thus gained a large experience in cervical tears and find it necessary to revise my previous notions of their anatomy. The cervix tears often even in spontaneous deliveries. The body of the cervix frequently tears, leaving the mucosa, internal and external, intact. Later such cervices show all the evidences of laceration, chronic inflammation, eversion, erosion, etc. Those lacerations which are open also show the separation of the muscle of the cervix at the sides, and the deep retracted portions of the wound must be pulled out and united, preferably with buried sutures. Our previous failures in cervical repair were, I believe, due to nonrecognition of this fact.

THE PERINEOTOMY

The technic of repair is one of the most important steps of the procedure. It is essential to have clear notions of the normal anatomy of the pelvic floor and how the structures are changed during delivery. The models (see illustrations) are intended to show these things. The head advancing through the hiatus genitalis (1) stretches the vagina radially and longitudinally—it also sometimes, wipes the vagina off its fascial anchorings, sliding it downward and

outward. (2) The head stretches the pelvic fascia over the levator ani, and between the rectum and vagina and the layer behind the rectum, also radially and longitudinally, and this also permits the rectum to be wiped downward and slid off its fascial attachments to the levator ani; (3) the head often tears, or overstretches the fascia over the levator ani, especially those bundles which hold the pillars of the muscle in position at the sides of the rectum, spanning the hiatus genitalis, and this permits the pillars to separate,—a real diastasis of the levator pillars resulting. The pathology is similar to that of the diastasis of the recti abdominales. This diastasis of the levator pillars and the wiping or sliding of the

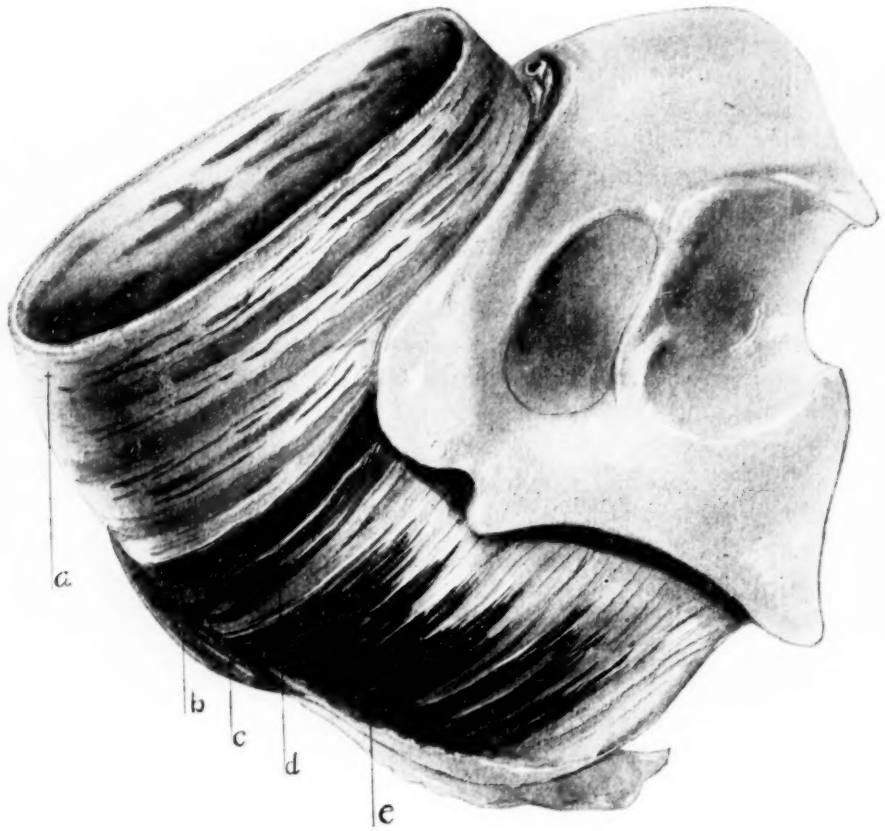

Fig. 1.—Partly diagrammatic to show the axial displacement, the distraction, and rupture of the fascia and muscles during the passage of the fetal head. *a*, Urogenital septum much distracted; *b*, usual site of rupture of levator ani; *c*, sphincter ani; *d*, levator ani pubic portion or "the pillars;" *e*, levator ani ischio-coccygeal portion.

rectum and vagina downward and outward are the essential features of most pelvic floor injuries have been, to my mind, the least noticed by current writers. (4) The tears in the levator ani muscle are usually due to improper treatment, and they occur least commonly near the insertion of the muscle on the pubic ramus (usually due to cutting by the forceps) and more commonly at the sides of the rectum, behind, near the raphé. (5) Labor always ruptures the urogenital septum, tearing it in all directions and also from its ramifications with the endopelvic fascia, both above and below the levator ani. (6) The fascia between the

vagina and bladder is also stretched or torn, radially and in a downward direction, tearing the vagina and bladder off its anchorage to the upper surface of the endopelvic fascia over the levator ani and posterior surface of the pubis.

Thus it is evident that most of the damage resulting from labor is due to injury, rupture, distraction and displacement of the fascia, and less to tearing of the muscles.

Prevention, therefore, aims to preserve the fascia in its normal position throughout the parturient canal, and, where the overstretching or rupture can-

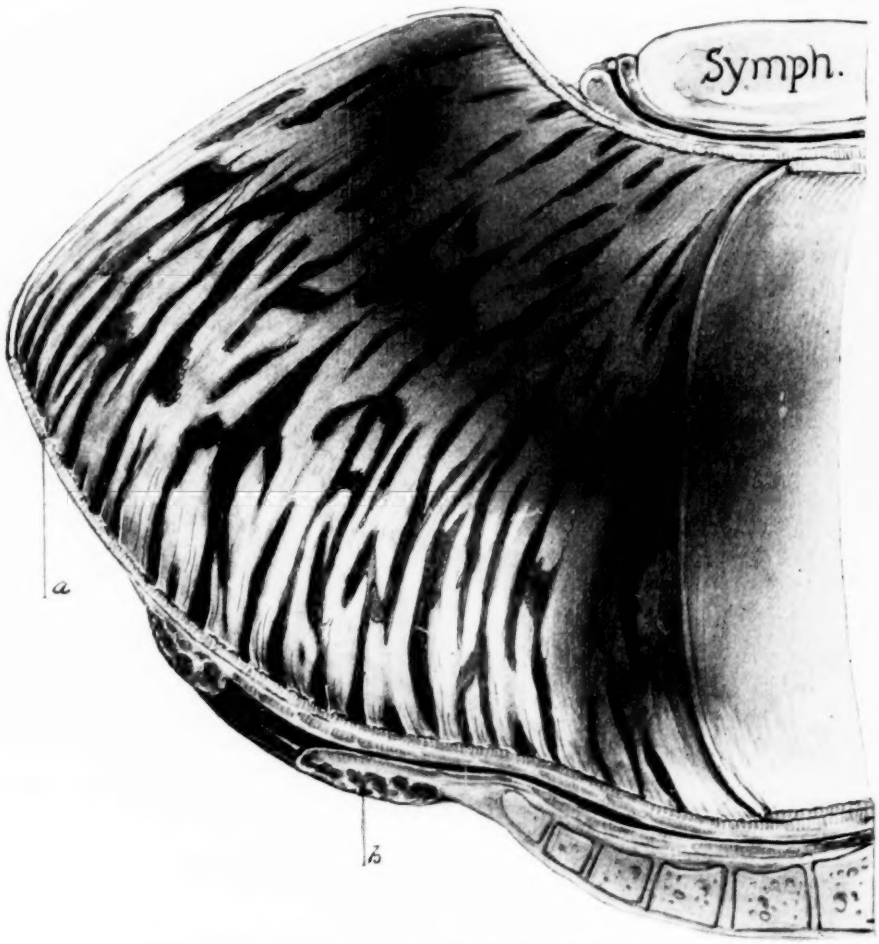

Fig. 2.—Purely diagrammatic, to show the interior layer of the levator ani fascia torn and distracted during the passage of the fetal head. *a*, Urogenital septum; *b*, sphincter ani.

not be avoided, to incise the structure at a spot where it can be repaired by suture.

We cannot do anything directly to save the pericervical connective tissues from radial and longitudinal overstretching and tears, but we can, indirectly, by avoiding all interference with the natural processes of dilatation of the cervix and restraining the natural powers if they are too violent. This means the avoidance of bags to hasten the dilatation, of manual stretching, of urging the

parturient to bear down before the head has passed the cervical barrier and especially avoiding pituitrin before complete opening of the cervix.

We can take direct action to save the fascial and muscular structures of the pelvic floor, in addition to practicing the measures just mentioned for preserving the connective tissues of the upper pelvis. By incising the fascia at its most vulnerable point, and reuniting it after delivery, we are almost always, not invariably, able to eliminate all damage to the pelvic floor.

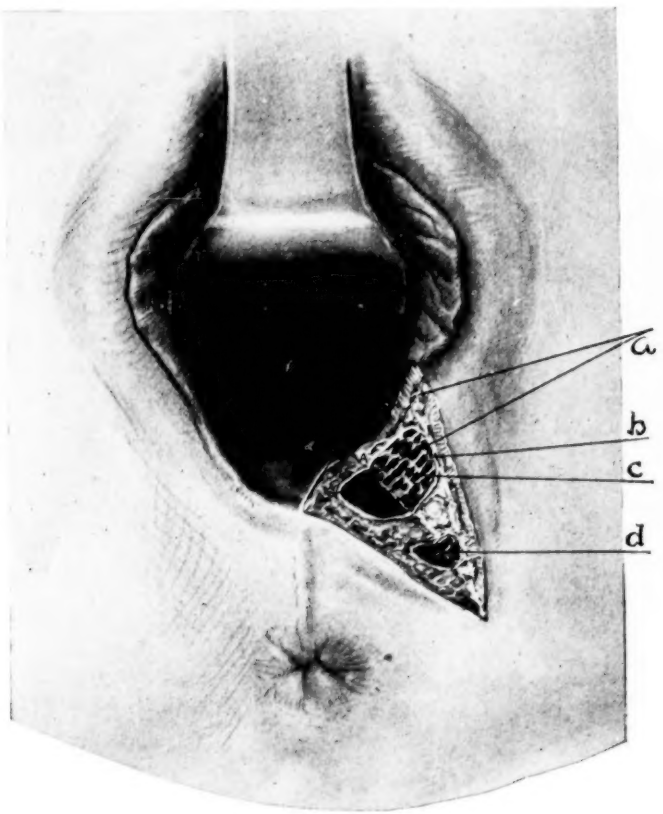

Fig. 3.—The perineotomy. Cut are the skin, the vagina, the urogenital septum, the outer layer of the levator ani fascia with its reflection over the deep transversus perinei muscle, the fascia over the levator ani both external and internal (the latter is called the fascia endopelvina). The portion of the fascia endopelvina between the levator ani pillars is called (by the author) the "intercolumnar fascia" and is shown at *A*. *a*, Urogenital septum; *b*, levator ani fascia; *c*, levator ani muscle or pillar; *d*, cut edge of deep transversus perinei muscle.

The first incision is through the skin and urogenital septum, exposing the pillar of the levator ani covered with the fascia endopelvina. Next the vagina is incised and with it the upper layer of the levator and fascia exposing the rectum, which is seen at the bottom of the wound covered with its fascia propria. Next the fibers of the fascia communicating with the urogenital septum are cut, which allows the perineal body with the sphincter ani and rectum to fall to the side opposite the cut. Simple episiotomy will not prevent injuries to the pelvic fascia. Where the disproportion between the head

and the pelvic floor is great, the muscular belly of the levator ani is also incised at a right angle to the length of the fibers. The models show these incisions better than descriptions.

Sometimes during the delivery the fascia tears and stretches more than we wish, but never so much that we lose the advantages of the preliminary incisions. By slow extraction we reduce this possibility very much. The repair is done with catgut, layer by layer, vagina, muscle, fascia, urogenital septum, subcutaneous fat and fascia and skin, all in anatomicosurgical fashion. Primary union is the rule and examination later shows that virginal conditions are usually restored.

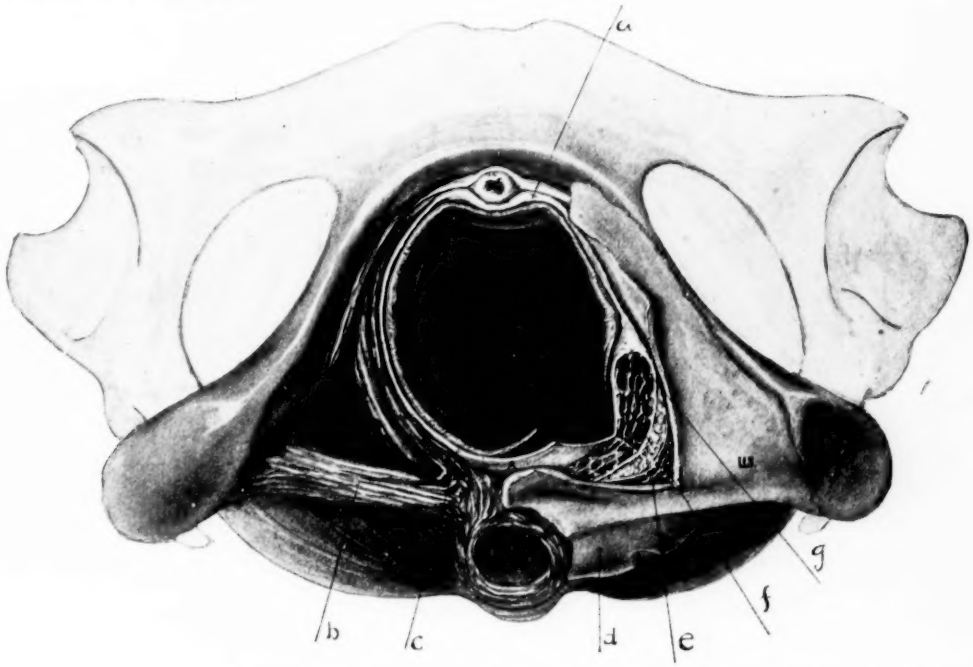

Fig. 4.—This model shows the dissection of the pelvic floor during the perineotomy. *U. S.*, Urogenital septum; *A*, the intercolumnar portion of the endopelvic fascia as it fuses with the urogenital septum in the centrum tendineum of the perineal body. On the left, the urogenital septum has been removed leaving the deep transversus perinei (enlarged) and showing the fusion of the levator ani fasciæ with the rectum. *a*, Vesicovaginal fascia (its destruction leads to cystocele); *b*, musc. transversus perinei profund. (exaggerated); *c*, fascia endopelvina portion called "Intercolumnar"; *d*, external layer of levator ani fascia. Floor of ischiorectal fossa; *e*, cut edge of deep transversus perinei muscle; *f*, levator ani pillar incised, pubic portion; *g*, fascia of levator ani.

Now, should virginal conditions be restored? Did not Nature intend women should be dilated in the first labor so that subsequent children will come easily? Are not the lacerations normal?

Labor has been called, and still is believed by many to be, a normal function. It always strikes physicians as well as laymen as bizarre, to call labor an abnormal function, a disease, and yet it is a decidedly pathologic process. Everything, of course, depends on what we define as normal. If a woman falls on a pitchfork, and drives the handle through her perineum, we call that pathologic—abnormal, but if a large baby is driven through the pelvic floor, we say that it is natural, and therefore normal. If a baby were to have its head caught in a door very lightly, but enough to cause cerebral hemorrhage,

we would say that it is decidedly pathologic, but when a baby's head is crushed against a tight pelvic floor, and a hemorrhage in the brain kills it, we call this normal, at least we say that the function is natural, not pathogenic.

In both cases, the cause of the damage, the fall on the pitchfork, and the crushing of the door, is pathogenic, that is disease producing, and in the same sense labor is pathogenic, disease producing, and anything pathogenic is pathologic or abnormal.

Now you will say that the function of labor *is* normal, that only those cases which result in disease may be called abnormal. Granted, but how many labor

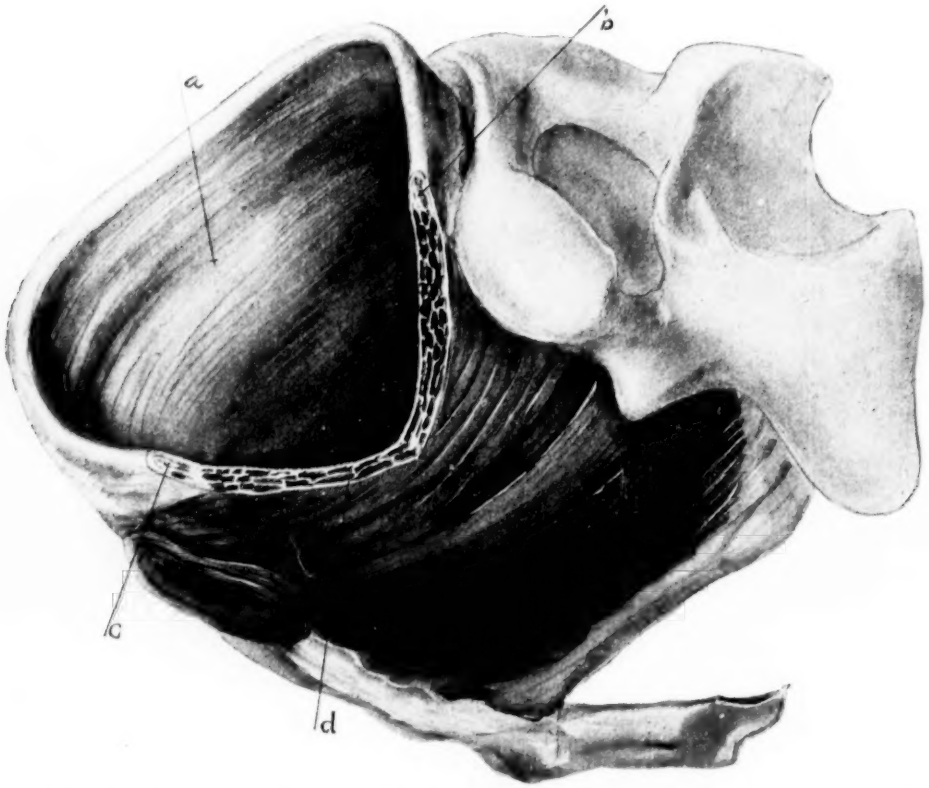

Fig. 5.—Condition of the muscles and fasciæ at time of exit of head after a deep perineotomy has been made. Note the short perineum, the anus pushed to one side, the intact fascia over the levator ani. Partly diagrammatic. *a*, Fascia over levator ani not distracted or torn; *b*, urogenital septum not distracted; *c*, urogenital septum not distracted; *d*, levator ani pubic portion or "pillar" incised.

cases, measured by modern standards, may be so classified? Sir J. Y. Simpson, said that labor, in the intention of Nature should be normal, but that in a large proportion of cases it was not so. If the proportion was large in Simpson's days, during the middle of the last century, it amounts to a majority today. In fact, only a small minority of women escape damage during labor, while 4 per cent of the babies are killed and a large indeterminable number are more or less injured by the direct action of the natural process itself. So frequent are these bad effects, that I have often wondered whether Nature did not deliberately intend women should be used up in the process of reproduction, in a

manner analogous to that of the salmon, which dies after spawning? Perhaps laceration, prolapse and all the evils soon to be mentioned are, in fact, natural to labor and therefore normal, in the same way as the death of the mother salmon and the death of the male bee in copulation, are natural and normal. If you adopt this view, I have no ground to stand on, but, if you believe that a woman after delivery should be as healthy, as well, as anatomically perfect as she was before, and that the child should be undamaged, then you will have to agree with me that labor is pathogenic, because experience has proved such ideal results exceedingly rare.

What are the factors that render labor so pathogenic? Dangers, immediate and remote, threaten both mother and child throughout.

First, for the mother. Infection is always a threat, even under the most ideal conditions. Virulent streptococci inhabit a large percentage of vaginæ, and if the second stage becomes too prolonged, if the bruising of the parts is too extensive, if the woman's resistance is worn down by too much suffering or by hemorrhage, they may invade the organism and prove fatal. The death may occur in a fashion that hides the cause from the unobservant accoucheur, e. g., a very mild sepsis, or even a single rise in temperature is shown, and, in the second week, death occurs from embolism.

Exhaustion is not infrequent in a second stage that may not be too long for a healthy woman, but in one whose nerve reserve is low, exhaustion may lead to immediate nervous shock, and later, pronounced neurasthenia. If the "twilight sleep" propaganda taught us anything, it showed the actual value of preserving the nervous strength of the parturient.

Of greatest importance, because of greatest frequency, is the damage to the pelvic floor and perineum; next comes the injury to the vesicovaginal fascia and then the lacerations of the cervix and the connective tissue supports of the uterus, the so-called uterine ligaments. It is not necessary before this society to enumerate the immediate and remote effects of this destruction of tissue.

The dangers of the second stage of labor to the child are much greater than one who has not studied the matter, may think. It may surprise some present to know that the following injuries have been caused by the forces of natural, spontaneous labor: fracture of the skull; rupture of the tentorium cerebelli; intracranial hemorrhage (numerous minute and large ones); retinal hemorrhage, abruptio retinæ, dislocation of the lens; facial paralysis; Erb's paralysis; rupture of the sternocleidomastoid muscle, already diseased, resulting in wry neck; fractures of all the long bones of all the extremities; rupture of the cord; tearing of the cord from its abdominal attachment, etc.

The most common dangers, however, and therefore the most important are asphyxia from abruptio placentæ or prolonged compression of the brain and intracranial hemorrhages. Brothers, of New York, found that 5 per cent of children died during labor. Holt and Babbitt, of New York, 4.4 per cent; Schultz, 5 per cent and 1.5 per cent in 24 hours from the trauma of labor, Kerness, of Munich, found 5.2 per cent and Potter, of Buffalo, had 4 per cent fetal mortality. A certain portion of these deaths occurs in natural, unassisted labor. How many babies are hurt and damaged in operative delivery cannot be determined, but their number is legion, and the same must be said of the

effects of natural labor. Any one who has thoughtfully studied the head of a child moulded by strong pains through the tight pelvis of a primipara will agree that the brain has been exposed to much injury. The long sausage-shaped head means that the brain has been dislocated, the overlapping bones indicate that the sinuses have been compressed with resulting cerebral congestion; the caput succedaneum evidences the pressure to which the brain was subjected. If there is a caput on the outside of the skull what of the inside? The punctate hemorrhages in the skin confirm the last-mentioned finding; the subconjunctival ecchymoses show us the possibility of hemorrhage in the retina. From outward visible evidences, therefore, we can deduce that the brain has suffered distortion, congestion, edema, compression and hemorrhages, but we need not rely on deduction alone. Clinically, if you listen continuously to the fetal heart tones, you will be convinced that the child is suffering, and autopsies bring the final proof of the above assertions. Neurologists for many years have pointed out the connection between epilepsy, idiocy, imbecility, cerebral palsies and prolonged hard labors. Observant obstetricians have known this for so long that it is an accepted fact. In 1917, Arthur Stein, of New York, reviewed the literature on the subject; he studied 5,562 cases in various homes for feeble-minded children, and comes to the conclusion given above. Indeed, although the statistics are meagre, they seem to show that instrumental delivery is safer than prolonged, hard, unassisted labor. Stein's article is well worth reading, as it quotes numerous accoucheurs and neurologists of scientific standing who support this view. One may well ask himself whether the brief and moderate compression of the head in a skillfully performed forceps operation, is not less dangerous to the integrity of the brain than the prolonged pounding and congestion it suffers from a hard spontaneous delivery. If a late forceps operation is done on a head and a brain already infiltrated with small hemorrhages, the results are worse, compounded.

Anoxemia (anaërosis, the beginning of asphyxia) of the child in the second stage is a not uncommon condition, but fortunately most children are born before the asphyxia becomes fatal. In the Chicago Lying-in Hospital, hardly a month goes by but that one or more infants die from this cause. Either the child is stillborn or dies a few minutes after birth, or dies within the week from atelectasis. Most so-called blue babies are simply atelectatic. The asphyxia may be primary—from separation of the placenta, pressure on the cord, tetanic action of the uterus, etc., or it may be secondary to cerebral compression or hemorrhage. Its beginning and progress may readily and easily be determined by means of the stethoscope, industriously applied during the second stage. Another result of asphyxia in labor is infection of the fetus. In gasping for air the child inspires vaginal mucus and later develops pneumonia or intestinal sepsis.

Among the late effects of prolonged labor on the child must be mentioned permanent disorders of the special senses, sight and hearing, due to hemorrhages into the nerve endings, the nerve itself, or its nuclei. Fetal deaths and all the complications are more frequent in primiparæ, as would be expected, even if the statistics and the history of primogeniture did not bear out the truth of the statement.

If we review all these things and if we admit that they occur even in so-called normal labor, we ask ourselves, are we today doing all that our refined obstetric art permits, to prevent damage and avoid disease of both mother and child? In other words, shall we depart from our old trusty, time-honored "watchful expectancy," i. e., waiting for distinct signs of distress on the part of the mother or babe before interfering—or should we anticipate these dangers and, as a routine, make the first stage of labor less painful and shorter and eliminate the second stage by a surgical delivery.

For the first stage, as stated before, we can do nothing safely except give narcotics, recommended in the form of a modified twilight sleep—unless we perform Cesarean section. It is surprising to me to receive requests from women for this method of saving them from even the pain of this part of labor. The most radical apostle of early surgical delivery is Potter, of Buffalo. In all cases, as soon as the cervix is fully opened (and oftentimes before), he completes the preparation of the soft parts manually and performs podalic version followed by immediate extraction. This practice has, and in my judgment, justly, evoked a storm of disapproval. In Potter's hands (perhaps) the operation is safe, but in less skillful hands there will undoubtedly be a long train of dead and damaged babies, ruptured uteri, and torn soft parts. The same may be said, though with considerable less force, to what I recommend for the obstetric specialist—the operation of "prophylactic forceps."

The radical interference with the mechanism of the third stage is intended to reduce the amount of blood lost, shorten the anesthetic period and diminish the danger of infection from retained blood clots, membranes and insufficient uterine contraction.

Now the writer freely admits that this method of treating labor is a revolutionary departure from time-honored custom and must have really sound scientific basis for recommendation. This it has.

First, it saves the woman the debilitating effects of suffering in the first stage and the physical labor or a prolonged second stage, and in the nervous inefficient product of modern civilization, this is becoming more frequently necessary. The saving of blood already referred to, has much to do with the quick and smooth recoveries I have observed in my cases. In the combination with morphine and scopolamine in the first stage, gas or ether in the second stage, and operative delivery, we have robbed labor of most of its horrors and terrors, and we ought to thus favor the increase of the population.

Second. It undoubtedly preserves the integrity of the pelvic floor and introitus vulvæ and forestalls uterine prolapse, rupture of the vesicovaginal septum and the long train of sequelæ previously referred to. Virginal conditions are often restored.

Third. It saves the babies' brains from injuries and from the immediate and remote effects of prolonged compression. Incision in the soft parts not alone allows us to shorten the second stage, it also relieves the pressure on the brain and will reduce the amount of idiocy, epilepsy, etc. The easy and speedy delivery also prevents asphyxia, both its immediate effects and its remote influences on the early life of the infant.

There are three objections to the innovation and one is a real one, but it will be, let us hope, only temporary. Prophylactic forceps will be made an excuse by unskilled, conscienceless accoucheurs, for the hasty termination of labor, not in the interests of the mother or babe, but for their own selfish ends. I fear that there are already too many forceps operations, and therefore, I hesitated long before I decided to publish this method. But I have always felt that we must not bring the ideals of obstetrics down to the level of general, the occasional practitioner—we must bring the general practice of obstetrics up to the level of that of the specialist. Let us trust each man to do honestly according to his limitations. For the one, watchful expectancy, for the other, prophylactic forceps.

The other two objections are, the possibility of infection and the dangers to the child from an improperly performed forceps delivery, brain injury and compression of the cord. If the woman has an evident infection or if there is a suspicious leucorrhea, the operation is contraindicated. In clean cases the matter of infection should not deter us. We practice a technic as painstaking as for laparotomy and have no fear of the results.

As for the forceps operation, in skillful hands the danger is *nil*. By means of the head stethoscope we are able to recognize danger to the infant from asphyxia and since the resistance of the soft parts is gone, there is no compression on the child's brain. We should not blame the operation for faults made in its performance.

The results of this new method of treating labor are all that one could wish for. As yet, no mother or baby has died; there has been no case of infection or cerebral hemorrhage. The babies have thriven, the mothers have not shown the exhaustion and anemia of former days. The restoration of the parturient canal has been always perfect—indeed, too nearly perfect. I have the impression that involution is quicker and more complete, that retroversion of the uterus is rarer, and all in all, the recovery much more rapid and satisfactory than with the older treatment.

426 East Fifty-first Street.

EXTRAPERITONEAL CESAREAN SECTION*

By John A. McGlinn, M.D., M.S., Philadelphia, Pa.

HAVING had considerable experience in the past twenty years with the Sänger and Porro Cesarean section in clean and infected cases, with but two maternal deaths, both moribund eclamptics, it is natural that I should view the claims made for the so-called newer operations with a certain degree of skepticism. The operation of extraperitoneal Cesarean section, however, is not new. It is only a revival of an operation which has been advanced and abandoned several times in its century of existence.

Jörg first advanced the operation in 1807 and was followed by Ritgen who operated in his first case in 1821. The operation was not completed; profuse bleeding compelled him to abandon the attempt and resort to the classical Cesarean. The child died shortly after birth and the mother died two days later from hemorrhage. In 1822 Dr. Physick proposed a similar operation, based on anatomic studies, but the operation was never performed. There is a record of a similar operation being attempted in France in 1823, but this also had to be abandoned and the technic changed to the classical operation, on account of the difficulties encountered.

We can understand the crying need of such an operation during the period prior to aseptic surgery, when practically every case of the classical operation resulted fatally from infection, and yet the operation was apparently abandoned and we hear no more of it until 1870. In that year Dr. T. G. Thomas conceived the idea of extraperitoneal Cesarean section and performed the operation on the cadaver. He afterwards performed the operation on a woman dying of pneumonia in an effort to save the child which, however, died shortly after birth. In 1875 Dr. Skene performed the first successful operation. Other operations followed and Thomas was able to report a series of cases with four living babies and a maternal mortality of 40 per cent. Nicholson states there were reported fourteen such operations with a maternal mortality of 50 per cent and a fetal mortality of 42 per cent. In 1892 Thomas discarded the operation for the classical Cesarean section with aseptic technic.

With the advent of aseptic surgery the operation was again practically abandoned though Frank continued at work on the problem. He desired to perfect an operation which would be safer than the classical Cesarean section in the infected case or in the case that had been in labor a long time. In 1906 he proposed a transperitoneal operation with the isolation of the general peritoneal cavity by attaching the parietal to the visceral peritoneum. Between 1892 and 1906 very little mention of the subject is found in the literature, the supposition being that the operation was discarded in favor of the classical operation with aseptic technic.

Subsequent to 1906 many Continental surgeons proposed varied technics for the true extraperitoneal operation and some modifications of Frank's trans-

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peritoneal method. We find, however, no reference to the subject in British or American literature until Tweedy's paper in 1911 and Nicholson's masterly review in 1914. Although Tweedy's paper was a clarion call to the British surgeons to adopt the operation, I have been unable to find another reference in British literature since that date. There is also a paucity of papers in American literature during the past ten years. The war hardly explains this lack of interest as the Continental literature is full of references during the same period.

Recently there has been a reawakening of interest in this subject and we hear many discussions of the advantages of the newer operations, which are as old as the operation itself.

Extraperitoneal Cesarean section operations can be divided into two general types.

a. The true extraperitoneal in which the peritoneal cavity is not invaded at any stage of the operation. This operation will be referred to in this discussion as the extraperitoneal.

b. The transperitoneal in which the peritoneal cavity is opened and subsequently isolated by suturing the parietal and visceral peritoneum and the uterus opened in this artificial extraperitoneal space. This operation will be referred to in this discussion as the transperitoneal.

The advantages of the extraperitoneal operation over the classical Cesarean section were set forth by Tweedy in a paper published in 1911. Speaking of the classical operation he stated; "He (the operator) knows that to obtain good results the operation must be performed before or within a comparatively short time of the onset of labor; that the membranes should not be ruptured; that frequent vaginal examinations introduce an element of danger which may act as a positive contraindication to the operation. Above all he knows that the wound is liable to imperfect healing. As proof of this one need only point to the number of deaths that have occurred from rupture of the uterus in subsequent pregnancies; to the device of introducing adhesions between the uterine and abdominal wounds; and finally to the radical procedure of sterilization which insures against another pregnancy."

Referring to the advantages of the extraperitoneal operation: "If it is true that the newer operation can be performed safely and becomes easier when the woman is far advanced in labor with the membranes ruptured and the lower uterine segment greatly thinned out; that the incision through which the child is delivered involves such a comparatively unimportant and bloodless part of the uterus that its rupture in a subsequent pregnancy would be a matter of little significance; that for practical purposes the operation is extraperitoneal; that there is no possibility of the formation of adhesions to cause intestinal obstruction or incarceration of the uterus; that because of a pre-existing sepsis suppuration of the uterine wound does not necessarily mean general peritonitis, then surely the operation is worthy of greater consideration than it has yet received from British surgeons."

B. C. Hirst (1913) sums up the advantages as follows:

"*First.*—The mortality of an extraperitoneal section should be the minimum in both clean and infected cases.

"*Second.*—The uterine wound is in such a position that even if it should leak or become infected the result is not necessarily disastrous.

"*Third.*—There can be no intraperitoneal adhesions with an abnormally high position of the uterus as the least disagreeable consequence.

"*Fourth.*—The convalescence of the patient is much more comfortable in every way.

"*Fifth.*—The abdominal wound is stronger, less disfiguring, less likely to exhibit hernia.

"*Sixth.*—If the uterine wound should burst in a subsequent pregnancy or labor, the accident is not so dangerous as if the wound were intraperitoneal."

He goes on to say: "I am not, however, arguing so much for the adoption of the extraperitoneal section in infected cases, as for the deliberate selection as the best of operations in clean cases."

He also decries the difficulty of the operation as an argument against its general adoption and yet in the same year he perfected and advocated a transperitoneal operation which has not, as fully, the advantages of the extraperitoneal but which is shorn of the difficulties of the latter.

DeLee states: "Viewed from the most modern viewpoints, the classical Cesarean section leaves much to be desired. In the first place, it is not safe when infection is present or only suspected, therefore it cannot always be used to obviate craniotomy on the living child: second, postoperative intestinal complications are frequent, and while seldom fatal, are always disturbing; third, peritoneal adhesions are often left, causing suffering and trouble later; fourth, the uterine scar may rupture in a subsequent labor; fifth, there is still a mortality of one to two per cent and higher in just those cases where one would like to adopt this method of delivery."

The disadvantages of the classical Cesarean section may be summarized as follows:

1. In the nonelective case the maternal mortality is higher than in the elective case. The mortality increases with the length of labor prior to operation; the rupture of the membranes; the number of vaginal examinations, and the attempts at vaginal delivery. The mortality is dependent upon infection, frequently peritonitis.

2. The frequency of rupture of the uterus in the site of the scar in subsequent pregnancies and labors.

3. The formation of adhesions between the uterine and abdominal wounds.

4. The unsightly abdominal scar and the possibility of incisional hernia.

5. Postoperative intestinal complications.

The advantages of the two types of extraperitoneal operation may be summed up as follows:

1. The peritoneal cavity not being opened or isolated from the field of operation, the danger from infection is less and therefore are better operations in the infected or supposedly infected cases.

2. If the uterus ruptures in the site of the incision in subsequent pregnancies or labors it is an accident of no material consequence.

3. There is no danger from the formation of peritoneal adhesions.

4. The scar is not unsightly and the possibility of incisional hernia *nil.*

5. There are no postoperative intestinal complications.

6. Hemorrhage during the operation is slight.

First.—In order to debate the first of these disadvantages and advantages it will be necessary to discuss briefly the subject of puerperal infection. It will not be necessary to discuss sources of infection other than through the birth canal. It is accepted that infections of the birth canal are implanted from without and increase in frequency with the number of vaginal examinations, the length of labor, the rupture of the membranes and attempts at manipulative or instrumental delivery. Wound infections of the vagina are not germaine to the subject. Infections may also be implanted in the uterus and the usual location is in the placental site. When infection gets into the uterus it may travel to the peritoneum through the lymphatics, the Fallopian tubes and in the case of Cesarean section through the uterine incision. It may extend into the pelvic cellular tissue, the body of the uterus, the pelvic veins, causing a thrombophlebitis, or into the blood stream causing a bacteriemia.

It must be admitted that the Sänger operation does not in any way combat infection in the frankly infected or potentially infected case. Further it increases the dangers by predisposing to general peritonitis both by the "spill" of the amniotic fluid and extension of infection through the uterine incision. The Sänger operation, followed by hysterectomy and the extraperitoneal treatment of the stump, fulfills every need while the infection is still limited to the uterus except for the "spill" during the operation.

To what extent does the extraperitoneal operation meet the indications to combat infection? Only in two ways, the "spill" from the uterus is extraperitoneal and if the uterine incision becomes infected, there is less danger of peritonitis provided the peritoneum has not been torn during operation, an accident which has frequently occurred. If infection occurs in the pelvic cellular tissue it is not so serious a matter as if it occurred in the peritoneum. This is true and yet patients die after pubiotomy from infections in this locality and as a matter of fact statistics show a mortality of 4 to 5 per cent from infection in the extraperitoneal cases. Some authors lay great stress on the advantage of drainage of the uterus through the uterine fistula by packing the uterine cavity with iodoform gauze and bringing one end out through the abdominal incision. Practically what advantage would this have over draining down hill instead of up hill by packing the uterus with gauze and bringing it out through the cervix? We have all learned how futile such treatment is. The extraperitoneal operation does nothing to combat infection if it is lodged in the placental site or has extended beyond it to any degree whatsoever.

The transperitoneal operation is even less efficient. I do not believe there is a method of attaching the parietal and visceral peritoneum that will absolutely prevent leakage of the "spill" into the peritoneal cavity. Again there is always the possibility that the attached peritoneum will be torn apart during the delivery of the child and the peritoneal cavity exposed. This has actually occurred in every case that I have witnessed, the intestines being seen in the upper angle of the incision. I feel sure that with proper technic the peritoneal cavity can be as effectually protected in the Sänger operation as in this operation.

If after delivery of the child the two layers of peritoneum are separated and attached to their position, there is little added protection against the spread of infection through the uterine incision other than that given by an infected peritoneum covering a more or less traumatized area of the uterus, for the peritoneum must be infected by the discharges passing over it and the uterus must be traumatized when its covering is detached.

If the "spill" from the uterus and infection of the incision were the only dangers to be met in the delayed or handled case then the extraperitoneal operations would practically solve every difficulty. They have no place in the frankly infected case any more than the Sänger operation has. When we elect the operation in the potentially infected case we are simply gambling on the presence and extent of infection.

Cragin in discussing the relative value of the two operations says: "That the classical operation of Sänger is a better operation in clean cases and that in infected cases the superiority of the extraperitoneal Cesarean section over the Sänger incision followed by hysterectomy after the removal of the child is still to be proved. Both the maternal and the fetal mortality of the Sänger Cesarean section at the Sloane Hospital are lower than those of the extraperitoneal Cesarean section as reported from Germany, hence the author feels justified in preferring the former."

Williams states: "As neither of these operations are available for use in infected patients, are more difficult to perform, and do not give better results than the classical Cesarean section, it is questionable whether they will permanently displace it after the novelty attending their employment has disappeared."

Second.—Sufficient cases of rupture of the uterus at the site of the uterine scar have been reported to make this a vital subject. To say, however, that if the rupture occurs in the lower uterine segment instead of the body of the uterus is an accident of no material consequence is a doubtful statement and not based on observed facts. The usual site of rupture of the uterus is in the lower uterine segment, just where the incision of the uterus is made in the transperitoneal section. In Lobenstine's thirty-seven cases of uterine rupture the mortality was 70 per cent. In Eversham's 140 cases the mortality was 45.8 per cent and in Scipiades' 97 cases it was 65.8 per cent. Why should rupture of the uterus in the lower uterine segment after Cesarean section show a smaller mortality than rupture at the same site without Cesarean and how in the light of these mortalities can it be regarded as an accident of no material consequence? As a matter of fact rupture of the uterus is an exceedingly dangerous condition no matter when, where, or how it occurs and no statement not backed by facts can prove it otherwise. It is yet to be proved that rupture of the uterus is less frequent after the transperitoneal operation than after the Sänger. Theoretically it should be more frequent.

Third.—There is no danger from the formation of peritoneal adhesions.

The formation of peritoneal adhesions in the Sänger operation is an unquestioned disadvantage of the method. This disadvantage is entirely eliminated in the extraperitoneal operation and practically entirely so in the transperitoneal.

Fourth.—The scar is not unsightly and the possibility of incisional hernia *nil.* This cannot be denied in the extraperitoneal operation when the Pfannenstiel or inguinal transverse incisions are used. Where the median or vertical lateral incision is used there is, of course, no difference between them and the ordinary incision for the Sänger operation.

Fifth.—There are no postoperative intestinal complications. There is no question that in the extraperitoneal operation peritoneal insult is avoided and there is no danger of intestinal complications other than might follow a normal labor. In the transperitoneal operation, however, while the danger is less than after the Sänger operation it is not entirely eliminated.

Sixth.—Hemorrhage during the operation is slight.

This may be true and many authors lay stress on this point, but it is not in accord with my observation or experience. Again many authors contravert this statement, giving hemorrhage as one of the disadvantages of the extraperitoneal operations. The operations which I have seen in this region have been bloody. During the war I had considerable experience wth gun-shot injuries in the prevesical space and I found bleeding not only profuse but difficult to control. If complete hemostasis is not obtained infection is likely to follow in the deep cellular tissue. I recall one case of infection following injury in this region with death resulting from peritonitis when autopsy showed absolutely no injury to the peritoneum.

In the transperitoneal operation while bleeding may not be more profuse than in a Sänger operation it is at times certainly more difficult to control. I recall seeing such an operation performed by one of our most skilled obstetric surgeons where the bleeding so obscured the field of operation that it took him ninety minutes to complete the operation while his average time for a Sänger or Porro operation is twenty minutes.

While the extraperitoneal operations have certain admitted points of superiority they also have certain disadvantages.

First.—The extraperitoneal operation is unquestionably more difficult and time-consuming than the classical Cesarean. Some authors hold that this is not a just argument against the general adoption of the operation. They maintain that the surgeon should master the technic and he should not allow the difficulties of the procedure to prevent his performing an operation so vastly superior to the classical section. Logically these advocates should all perform the Dührssen-Sohn's operation, which, while the most difficult, is at the same time most truly extraperitoneal. The difficulty of the operation is a disadvantage. Skill can only be acquired on the living subject and the American surgeon hesitates to acquire skill at the price of the life of his patient. We see all through the history of the operation the tendency towards the simplification of technic and of late the advocacy of the less scientific but easier transperitoneal operation.

Second.—On account of the time it takes to perform it is not applicable in the rare case when speed is necessary to save the child.

Third.—There is danger of injury to the bladder and ureter. This accident has been referred to frequently in the literature.

Fourth.—It is not applicable in cases of placenta previa.

Fifth.—It has no place in the frankly infected case.

Sixth.—In clean cases where a positive indication for section exists it is not possible to repeat the same operation (extraperitoneal) in the majority of cases.

Seventh.—Mortality and morbidity of the operation. Latzko collected 150 cases with a general mortality of 7.3 per cent and a mortality from infection of 5.4 per cent. In Jeanin's 148 cases the general mortality was 7.45 per cent, while the mortality from infection was 3.04 per cent. In Lewis' 102 cases the maternal mortality was 8.8 per cent. In Latzko's and Jeanin's combined cases the morbidity was 30.7 per cent of which 25 per cent were due to infection.

In discussing this operation it is interesting to go back a few years and read what standard textbooks of obstetrics said of it.

Dorland in 1901 makes no mention of it.

Webster in 1903—"It should have no place in obstetric surgery at the present time."

Norris in 1903 gives nine lines to the subject. "The necessity for laparo-elytrotomy can scarcely be said to exist."

Hirst in 1899, speaks of laparo-elytrotomy as an operation that is no longer justifiable.

What is the status of the operation at the present time?

Theoretically in clean cases the extraperitoneal operation is the ideal operation, but its disadvantages overshadow its advantages. The transperitoneal operation is superior to the classical Cesarean section for the reason that the resulting adhesions are in a situation less likely to give trouble. The Beck operation is superior to the transperitoneal operation, as it has all the advantages of the former and none of its difficulties.

In the frankly infected cases the operations are contraindicated.

In the suspected cases the operations have a place. It is to be remembered that they only protect the peritoneum against infection from the spill of the amniotic fluid and extension of infection through the uterine incision. When they are selected instead of the Sänger incision followed by hysterectomy we are simply gambling with the extent and distribution of the infection. It must also be remembered in this connection that all cases of peritonitis following Cesarean section are not uterine in origin.

My own feeling is that the Beck operation with thorough protection of the peritoneal cavity and perfect peritonealization of the uterine incision is superior to the transperitoneal operation as a routine procedure. While theoretically it is not as efficient as the extraperitoneal method, practically, on account of the many disadvantages of the latter, it is the better operation.

In conclusion: The operation is over a century old. The same discussions that we are hearing now were heard in 1807, in 1827, in 1870, in 1906 and in 1913. There has hardly been a new thought advanced, pro or con, except variations in technic. The operation has never been generally accepted and it never will be. While it has many advantages they are overshadowed by its disadvantages.

It is a laudable object to perfect technic to reduce mortality, but our real problem is to so raise the standard of obstetric knowledge and practice as to make the operation unnecessary.

113 South Twentieth Street.

CRANIAL AND INTRACRANIAL BIRTH INJURIES*

By Harold Bailey, M.D., F.A.C.S., New York, N. Y.

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MANY are engaged in a movement which has for its object prenatal care. Maternity centers have been established in the larger cities and they are popular for their motive appeals to all classes. The basis of their work is a two-fold desire, the protection of the mother during her pregnancy and labor and the assurance that a healthy and normal child will be brought into the world.

If the report of the Health Department[1] for the week ending March 20, 1920 is examined, it will be found that of the total number of those born—living and stillbirths—there were 3.7 per cent born dead and of those living 3.8 per cent died within the first month. These figures combined, compare closely with the figures from the Johns Hopkins clinic[2] which show 7 per cent of infant deaths in 10,000 labors and with the Sloane Maternity[3] of 7.2 per cent in the same number of cases. The latter figures in both instances being compiled from the infant deaths from the seventh month of gestation to fourteen days after delivery.

In a series of 14,468 births, with a stillbirth rate of 3.6 per cent, J. C. Edgar[4] found that of the 341 cases in which the cause could be determined 110 or 32 per cent had obstructed or protracted labor. McQuarrie[5] in a small series found that the stillbirths were due in 37½ per cent of the cases to trauma.

In this paper I propose to call attention to the fact that a considerable proportion of stillbirths and early deaths are due to injury to the head of the infant and to suggest that in a few instances the lesions lend themselves to treatment that might lessen in some degree the early death rate and lower the morbidity in those infants that now survive.

Several of the earliest obstetricians recognized the significance of cranial injuries as a cause of stillbirth. Marceau in 1695 and Smellie in 1752 and Litzman in the latter part of the nineteenth century reported cases of depression and fracture of the skull in the parietal region. Rosinski,[6] in 1893, tabulated all the cases that he could find in the literature, which proved to be 38; and he added 5 cases of his own to the discussion of this subject.

As early as 1837 Evory Kennedy[7] called attention to the brain lesions, especially edema and hemorrhages, occurring in certain newborn and very young infants; and about 1851, Weber[8] and in 1853 Hecker[9] described in detail the meningeal and cortical injuries occurring at birth.

That some of these bleeding cases survived and suffered from the effects later in life was first made clear by Little.[10] In 1861 he read a paper before the London Obstetrical Society entitled: *On the Influence of Abnormal Parturition, Difficult Labors, Premature Birth and Asphyxia Neonatorum on the Mental and Physical Condition of the Child Especially in Relation to Deformities.* The full title of the paper is cited because it shows the manner of

*Read at a meeting of the New York Obstetrical Society, April 13, 1920.

his explanations for the paraplegias and idiocy and other brain symptoms occurring later in childhood. He gives the birth history in some 50 cases and states that added to 200 cases that had applied at his hospital for orthopedic treatment there were a great many in idiot asylums over which he had no control. He found that some of the infants had been born by the breech, some were premature, and in others it had been difficult to start respirations. He laid great stress on the fact that asphyxia would considerably increase the pressure in the cortical veins and he believed sufficient to rupture them especially in the premature. Correlated with the autopsy findings that he mentioned in his paper, he entirely established the connection between brain hemorrhage at birth and infantile spinal paralysis, now often called Little's disease.

In 1885 Sara J. McNutt[11] collected 10 cases of autopsies on infants dying within 5 days of birth and with the obstetric history showed that intracranial hemorrhage was a considerable factor in producing mortality in the first days of life. Her contribution is very important for the following reasons: It established in a group of cases the pathologic picture and from the point of etiology completes Little's paper. Evidence was produced that delivery by the breech might be accompanied by bleeding over the vertex as it occurred in three of her cases. Finally, the collection of such a group now assumes importance for in each of these ten cases time enough existed between birth and death for surgical intervention to have taken place and perhaps offers evidence to us some thirty-five years later, that we must move in that direction.

In 1892, Herbert Spencer[12] found in 130 autopsies in the newborn, 53 or 40.7 per cent had cerebral hemorrhage. Archibald[13] in 1909 found 43 per cent of 74 cases with intermeningeal hemorrhage. Warwick[14] in 1919 showed 50 per cent of hemorrhages in 36 deaths in infants stillborn or dying early; and our own series shows 40 per cent in 100 cases in which the skull was opened.

Assuming that the literature offers proof that cerebral injuries are not infrequent, we may turn to those papers which deal with treatment.

Kerr[15] mentions the report of Boissard of a case trephined in 1877. This is the first decompression operation on the newborn. It was performed for fracture of the parietal bone with intracranial pressure sypmtoms, and it was a success.

In 1901, Kerr[16] suggested a method of treatment for the spoon-shaped depressions occurring in the frontal and parietal regions. The skull was squeezed in the longitudinal direction, thus increasing it in the lateral dimension and perhaps at the same time producing more pressure within. He mentions three successes by this method. As a surgical procedure it is illogical, for the danger from such an injury is intracranial pressure and hemorrhage, and the depression may be accompanied by fracture—conditions that would not be helped by further increase of pressure within the skull.

Cushing[17] in 1905, advocated the adoption of the same principles of treatment in the cerebral bleeding of the newborn that are made use of in the adult. He reported four cases with definite symptoms in which he decompressed the skull by a large osteoplastic flap, which was turned back in the parietal region. Two of the cases recovered and two died. The history of these babies together with the two autopsy reports forms a foundation for future work in this subject.

Tweedy[18] in 1908, advised the treatment which was in vogue in the Dublin Maternity for the depressions occurring about the coronal suture. Apparently his advice is meant to apply only to those depressions which occur without immediate symptoms of intracranial pressure. He makes an incision over the dent and boring through the bone with the end of a vulsellum forceps, inserts the sharp end under the bone and pulls it up, thus reinverting it. Kosmak[30] has recently devised an instrument for use in this procedure.

ANATOMIC AND OBSTETRIC CONSIDERATIONS

Little made it clear that some of the children showing lesions attributed to injuries at birth were breech presentations, and that there were other factors beside head pressure in difficult labors. He pointed out that vessels in the cortex and other parts of the body are especially fragile and quoted Weber and Hecker as testifying to the frequency of minute hemorrhages in other organs besides the brain in stillborn infants. The congestion from asphyxia alone he believed was enough to rupture cortical vessels in full term children.

It is a fact noted by many that these stillbirths have hemorrhages in other viscera. Neglecting the explanation of congestion, Green[19] and also Warwick stated their belief that these cases represented deaths from a condition identical with that known in the first week as "hemorrhage of the newborn." This explanation is not in accord with our theories concerning the disease and it seems a little far-fetched, when in nearly all of these cases there is an asphyxia that dams back the blood in the veins and increases the pressure to an extent that would produce exudation.

Cushing agrees with Little that asphyxia neonatorum may lead to rupture of the cerebral vessels, just as it sometimes occurs in asphyxia from the spasms of whooping cough. McNutt's three breech cases with cerebral hemorrhage had the extravasation only over the vertex. While this is a significant fact as regards asphyxia, of course it is not conclusive, for the bleeding in this position might be due to concussion from the contact of the head with the pelvic bones in rapid withdrawal.

In hemorrhages occurring in the small and premature, we must recall that the cranial bones are poorly developed and that there are wide gaps at the suture lines. On first thought it would seem that there was little probability of cortical hemorrhage, for the cavity is more distensible; however with these bones loosely joined they are more easily made to overlap and thus cut the veins leading to the longitudinal or lateral sinuses, or even to injure the sinuses themselves. Virchow first pointed out that the veins of the cortex have little support as they enter the sinuses. Cushing also dilates on this anatomic condition. If these veins are unprotected for even a minute distance as they empty their current into the larger channels, it is evident that they may very easily be ruptured by marked moulding of the head.

In cases where the pelvis is flattened, the symphysis being 5 cm. deep in front and the sacrum 12 cm. in the back, the head cannot advance uniformly through the cavity. Usually the anterior parietal advances while the posterior bone is retarded. As a consequence the lower anterior bone moulds over the

one on the opposite side that is engaged against the sacrum. If the posterior bone advances first, the moulding becomes extreme. Edgar[20] says: "Presentation by the posterior parietal bone is serious because the head is wedged on the symphysis pubis. * * * Great pressure is exerted on the posterior bone by the sacrum, causing a depression in the bone. Sometimes this depression is spoon-shaped. It is quite likely that the brain has been injured." (Fig. 1.)

The sharp edges of the bones undercut the vein tendrils as they enter the sinus on the opposite side of the midline. Such a hemorrhage would be subdural, but might by dissection become subarachnoidal and circulate with the cerebrospinal fluid over the medulla and into the cord. As so many of the autopsies show blood in these places, it seems probable that this accident due to synclitism and moulding, in a flat pelvis, is not infrequent.

Fracture of the skull producing hemorrhage usually means the rupture

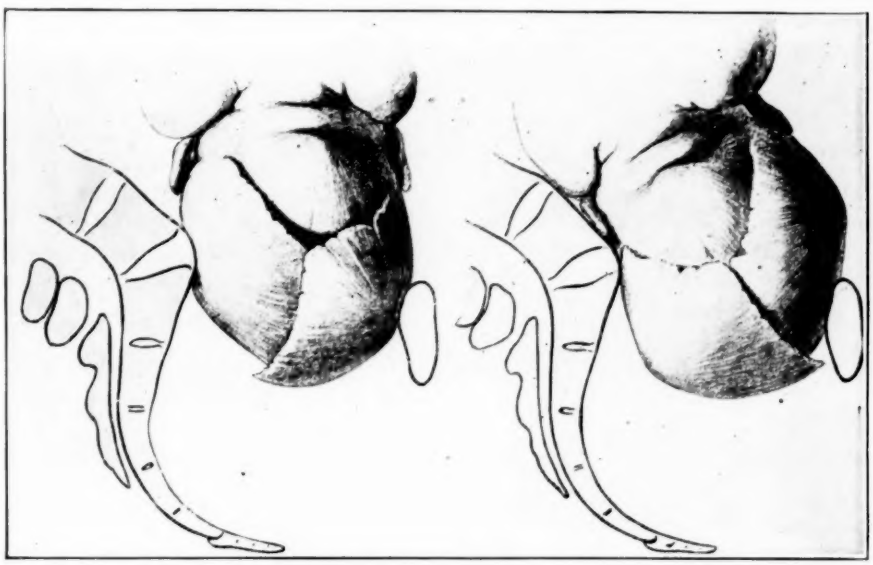

Fig. 1.—Synclitism with overlapping of the parietal bones. Apt to occur in flat pelves and may lead to rupture of the cortical veins as they enter the longitudinal sinus. (From Bumm, Grundriss zum Studium der Geburtshilfe.)

of a meningeal vessel. This condition seems to be rare, perhaps because of the loose attachment of the dura to the bone in the newborn. Poor application of the forceps or brutal force are the two conditions that might give rise to a crushing lesion such as is pictured by Rosinski. Of course, the close locking of the blades and traction against an obstacle would cause the parietal bones to overlap and the hemorrhage might be produced as outlined above.

The bleeding from the surface of the cortex is often held beneath the pia and may produce considerable damage even when it is of slight extent, if it is located near the cortical centers. (Figs. 2 and 3.)

Hemorrhage into the ventricles may occur from rupture of the chorioid plexus. It occasionally occurs without bleeding elsewhere in the brain. It was present in one of our cases, where the delivery was by Cesarean section, and Osler found it in a case of an unborn child of a woman dead from typhoid.

In Spencer's series there were 15 children delivered by the forceps. Of 12 that were alive when the forceps were applied, there were 11 that had intermeningeal hemorrhage and one with intercerebral bleeding. Twenty of his 53 cases were delivered by breech extraction, and there were 13 that were normal vertex presentations.

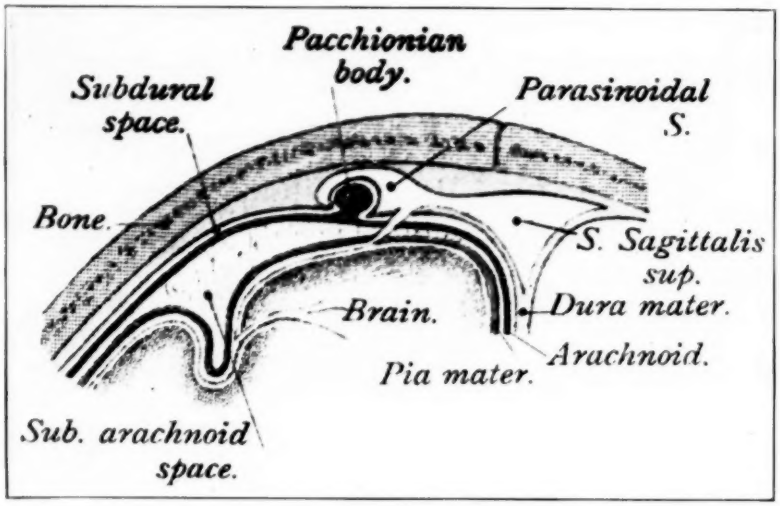

Fig. 2.—To show the subarachnoid space where the bleeding often occurs. (From J. Ryland Whitaker: *Anatomy of the Brain and Spinal Cord.*)

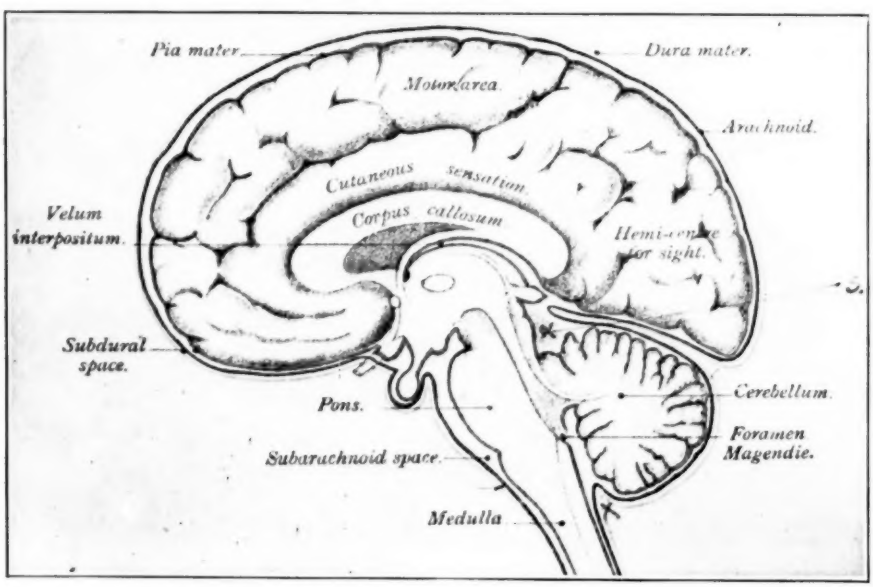

Fig. 3.—To show the relationship of the arachnoid at the base of the brain and to indicate the location where considerable blood may collect beneath the tentorium. (From J. Ryland Whitaker: *Anatomy of the Brain and Spinal Cord.*)

Margaret Warwick's series of 18 cases from the University of Minnesota Hospital are very accurately described. They are particularly interesting because 16 were born alive and of these, 12 were normal births with spontaneous

TABLE I

CLASSIFICATION OF FORTY CASES OF CEREBRAL HEMORRHAGE

AUTOPSIES FROM THE MANHATTAN MATERNITY HOSPITAL

HOSPT. NO.	STILL BIRTH	AGE AT DEATH	LOCATION	HEMORRHAGE IN VISCERA	ASPHYXIA	NOTES
FORCEPS DELIVERIES						
62	*		Cerebral	.....	yes	Median
98	*		Diffuse	Liver, lungs	yes	High
106	*		Diffuse over cortex and in Vent.		yes	High
111	*		Diffuse over cortex	Punctate lungs,	yes	.....
193	*		Over temporal region	lungs, heart, kidny.	yes	.....
198		36 hrs.	Over vert. Occip. and frontal	viscera, punctate	yes	.....
219	*		Diffuse in pia; marked over cerebellum	thymus, heart, lung	yes	.....
250		5 days	Extra and subdural and cerebellar	.....		High
251	*		Dural right side	.....	yes	Median
BREECH DELIVERIES						
67	*		Diffuse cerebral	.....	yes	Version
74	*		Diffuse cerebral	.....	yes	
78	*		Diffuse Marked occip. lobe	.....	yes	
99	*		Diffuse over entire cortex	.....	yes	
102	*		Diffuse meningeal	lungs, thymus	yes	
134	*		Over both temp. lobes	.....	yes	Premature
192	*		Multiple base and in pia	viscera	yes	
152	*		Occipital region	viscera	yes	
93	*		Diffuse in pia		yes	Version
141	*		Cerebral multiple		yes	Version
151		14 hrs.	Epidural over parietal. Diffuse in parietal lobe	viscera		Version
NORMAL DELIVERIES						
64		20 min.	Diffuse over cortex	Punctate viscera		Dry labor
88	*		Diffuse cerebral	Punctate lungs	yes	
101		2 days	Diffuse cerebral	.....		
117		1 day	Diffuse in pia	heart, thymus		Premature
126	*		Diffuse cerebral	lung, thymus	yes	
133	*		Diffuse in cortex	.....	yes	Premature
158	*		Diffuse, rupt. long. sinus	lungs, liver, etc.		Macerated
171	*		Diffuse, espec. right parietal	.....		
175	*		Meninges and ventricles	.....		Syphilis
196	*		Lateral ventricle	Punctate viscera	yes	Dry labor
191		4 days	Diffuse over occip. and parietal	" both lungs		Precipitate
181		36 hrs.	Diffuse. Thrombosis sinuses	in mucous memb.		
200	*		Diffuse, pia-arachnoid	viscera	yes	
223		3 days	Diffuse, espec. over parietal	punctate lungs		
215	*		Diffuse, espec. occip. region		yes	
234	*		Diffuse pia-arachnoid	lungs, liver	yes	
236		3 hrs.	Diffuse pia-arachnoid espec. over r. occip. parietal	lungs, thymus		
CESAREAN SECTION						
66	*		Both lateral ventricles		yes	
DELIVERY NOT KNOWN						
162	*		Hemorrhage in cerebellum	punctate lungs		
231	*		Diffuse parietal	viscera	yes	

establishment of respiration. There was only one infant delivered by forceps. Six of the hemorrhages were over one hemisphere. This finding upholds Cushing's belief that the hemorrhage is frequently limited to one side. There were 7 cases with the bleeding over the cerebrum and cerebellum and one case had, in addition, bleeding into the ventricles. There were two cases that had only dural bleeding and 2 with bleeding into the ventricles only.

In reviewing the Manhattan Maternity autopsy records in the last 185 autopsies, there were 100 in which the skull was opened and definite mention made of the findings. Of the 85 in which the head was not opened, 35 were so macerated or poorly preserved that it was deemed futile to proceed, and of the remainder it was apparently considered unnecessary to examine the brain because of such gross abnormalities found in other organs.

In 100 cases, dating from 1914, there were 40 cases with cerebral hemorrhage. There were but 10 that were not stillborn. One case lived 20 minutes, one 30 minutes, one three hours, and the others from 14 hours to 4 days. It is of note that of the babies that lived only 2 were forceps deliveries. Two were premature and 5 were normal births. In the entire series 9 were forceps, 11 were breech extractions, five of these being preceded by version; one was a Cesarean and 17 were normal deliveries. There were 2 cases in which the histories could not be found.

The hemorrhage in 18 cases was described as diffuse, and I think in most instances this meant over the cerebrum; 11 were noted as being especially marked under one bone; 2 were diffuse and in the ventricles, 2 were in the ventricles alone; 1 was a diffuse meningeal hemorrhage with thrombosis of the sinus; 2 were in the cerebellum; 2 were in the pia and 2 were in the dura.

A REPORT OF FIVE RECENT CASES OF CRANIAL AND INTRACRANIAL INJURY

CASE 1.—Female child of B. K., born on the Bellevue service Oct. 5, 1919. The mother had a flat pelvis and had been in labor two days when brought to the hospital. Forceps had been applied to the floating head. A version and breech extraction was performed and the operator was assisted by the house-surgeon pushing the head into the pelvis from above. The delivery proved to be remarkably easy and the child passed through the pelvis with great rapidity. On delivery a marked depression, of the spoon-shaped variety, was present in the left parietal bone, a little in front of and above the eminence. There were no signs of intracranial injury. As this depression was almost over the center of the Rolandic area, where scarring or injury would almost certainly be followed by paralysis, it was decided to elevate the bone. Eight hours after delivery an incision was made for a distance of 5 cm. directly over the dent, and the pericranium was incised. With the sharp end of a vulsellum forceps, by a drilling action a hole was bored in the bone. There was some compression of the cranial contents, for the dura was in close contact with the bone and it was punctured. Clear fluid came out under pressure. The hook was inserted under the bone and on lifting, the bone assumed its normal shape at once with a spring-like action. The pericranium was repaired with catgut and the skin with silkworm-gut. The baby showed no reaction and was normal until the fifth day when the skin wound broke down. The resulting wound was washed out daily and became at once a clean granulating surface. About this time a series of infections occurred in the nursery and later caused the death of a number of infants from a specific organism—the Streptococcus hemolyticus. On

the eleventh day, when the wound was nearly healed, the baby had a rise of temperature and died on the fourteenth day after delivery. On the day of its death a blood culture showed the organism mentioned. (Fig. 4.)

Case 2.—Infant of N. B., born on the Bellevue service Oct. 18, 1919. The baby presented in left occiput posterior position and was delivered by forceps using the Scanzoni procedure. As the head reached the outlet there was some difficulty in securing rotation. On delivery the left frontal bone had a spoon-shaped depression. Although there were no signs of intracranial injury the depression was raised by the method of Tweedy as described above. The wound healed by primary intention and the child gained weight in the usual manner. It was discharged with its mother on the tenth day.

Fig. 4.—Spoon-shaped depression in the left parietal bone produced by contact of the promontory of the sacrum with the after-coming head in breech delivery. (Drawing.)

Fig. 5.—Spoon-shaped depression of the left frontal bone produced by contact with the spine of the ischium in instrumental rotation of a head deep on the pelvis. (Drawing.)

As to the seriousness of spoon-shaped and other dents of the skull, the only available statistics except for occasional cases are those of Schroeder.[21] In 65 cases 34 per cent were stillborn, 15 per cent died and 50 per cent remained alive. In cases of meningeal injury the symptoms, being mainly those of increased pressure, are frequently delayed and in some instances do not appear until the second or third day. Case 4 will serve to illustrate this point. (Fig. 5.)

Case 3.—Intracranial injury. Male child of J. M., born on the Manhattan Maternity service March 1, 1920. Weight at birth 9 pounds 8 ounces. Forceps were applied for an anterior position of the occiput well engaged in the pelvis. The procedure was difficult, and even after the birth of the head the shoulder delivery was delayed and considerable traction was made on the neck of the child. Respirations were started by the Prochonik method of resuscitation. The child's face became swollen on the right side but the infant was sent to the ward without evidence of any other injury. In the morning some 12 hours later, it was found that there was a right facial and a right arm paralysis. The baby did not cry and could not be made to cry although its respirations were normal. It could not swallow or nurse and fluid placed in its throat with a dropper returned at once through the mouth. There was marked tension in the fontanelle. There was a slight groove-shaped depression on the posterior part of the left parietal bone above the eminence.

While at first glance it seemed that the child had a peripheral facial paralysis and an Erb's arm palsy, the above symptoms indicated that there was decided intracranial pressure. On close observation it was seen that the eye of the child remained closed and although there was some swelling of the upper part of the face it seemed as if this paralysis was due to a cortical lesion. The reflexes of the arm were not only present, but they were exaggerated. This was markedly so of the supinator reflex. The legs were not affected so far as could be determined.

The case was seen in consultation with Dr. A. M. Wright and he verified the above findings. He stated his belief that the skull should be opened to

Fig. 6.—Operative site, after healing.

relieve the symptoms of intracranial pressure, and he felt that the focal symptoms present pointed to a hemorrhage in the middle of the left Rolandic area.

The cranium was opened by a large osteoplastic flap of the left parietal bone. The depression in the bone was disregarded and the angle of the Rolandic area was laid out by folding a paper to 67½° and marked on the shaved skull. Hemorrhage was controlled by a catheter tied around the head just above the eyes. In making the incision into the bone the knife entered the coronal suture and punctured the dura. Great difficulty was encountered in cutting the bone with scissors, as the blades would separate. After a considerable opening had been made the bone was lifted and the remainder of the bone incision was comparatively easy. In about the center of the dura, then

exposed, there was a dark area. The dura was incised and at once there was a decided hernia of the brain, the cortex rising well above the bone. About the middle of the exposed area and as far as could be judged at the center of the fissure of Rolando there was a clot about 2 by 3 cm. lying directly over the cortex. This was washed with warm saline. The question arose as to the necessity for decompressing the opposite side in order to reduce the hernia. Irrigation with the warm salt solution and the equalization of the pressure gradually caused the brain to recede somewhat. The dura could not be united so the entire flap was replaced and the skin and pericranium were brought into apposition with many silkworm-gut stitches. The posterior and upper

Fig. 7.—Twenty-five days after operation. Note the raised area in the skull, the facial paralysis still remaining and the typical arm position.

portion of the flap was well above the level of the rest of the bone. The infant had only a small amount of ether by the drop method.

The child was fed by gavage, and after a few days the greater part of the facial paralysis cleared, he cried normally, and was able to nurse. Gradually some motion and strength was evidenced in the flexor group of muscles of the forearm, which later became spastic. The child lost over a pound in weight during the first days, but gradually gained a little. The wound healed by primary intention except for one stitch from which a little clear serum exuded on the tenth day. This healed promptly. (Fig. 6.)

The picture presented (Fig. 7) was taken on the twenty-fifth day after operation. At discharge on the thirtieth day, the upper arm showed a typical paralysis of the Erb type with all reflexes absent. On crying, the drooping of the right angle of the mouth showed that the facial paralysis had not entirely cleared.

The weight of the child was stationary at 8 pounds, 5 ounces, and it was necessary to put it on a formula, as the mother had but little milk.

There was a hard scar-like induration in the sternomastoid muscle of the left side. This induration was first apparent about 20 days after birth. The child will be followed carefully to watch its development. (Figs. 6 and 7.)

CASE 4.—Illustrating spoon-shaped depression with fracture and intracranial injury. Infant McC., born at the Manhattan Maternity, February, 1920. This case was delivered by the forceps for an occiput posterior position. At birth there was some flattening of the right parietal bone, but this was obscured within an hour by a well marked pericranial hemorrhage of the type known as cephalhematoma. The baby had a sighing type of respiration and could not swallow or nurse. On the second day it had tense fontanelle and a peculiar weak cry, which is common with cerebral irritation. There were no localizing paralysis, but on the fourth day it developed convulsions and died on the following day. Autopsy showed a fracture beneath the spoon-shaped dent with rupture of a small branch of the middle meningeal. There was also subarachnoidal bleeding and some blood beneath the tentorium.

This case is very interesting, for if it had been operated by the method of Tweedy, as were the first two cases in this series, it would not have been helped. Here it was evidently necessary to decompress the skull to relieve the pressure. This case antedated Case 3 in which the decompression was done, and the study of its history and autopsy findings opened our minds to the necessity for action.

CASE 5.—Infant McA., stillborn on the Manhattan service March 14, 1920. The head advanced to midpelvis in a right posterior position and then forceps were applied. At birth the child did not breathe, but the heart beat strongly for 20 minutes. The usual methods of resuscitation were not successful in starting respirations. On autopsy there was found a hemorrhage within the dura of considerable extent that had found its way over the temporal and posterior fossæ, and some blood was also found on the right side and beneath the tentorium cerebelli. There was no subarachnoidal or cortical hemorrhage.

This case is cited because it is so typical of forceps trauma and gives rise to speculation as to the advantages to be gained from rapid decompression through the coronal suture line while respiratory efforts are continued by means of mechanical apparatus.

SUMMARY AND CONCLUSION

The results from decompression operations of the large osteoplastic flap type are not good. Cushing[22] operated nine cases with but four successes. With the exception of our case all other cases have been fatal. Taylor[23] reported 2; Seitz,[24] 1 (?); 2 by F. T. Murphy[25] and one each by Ballock[26] and Hubbard.[27] There are, however, four successful cases of decompression by another method. Simmons described a method patterned after that used by Boissard for lifting the bone in dents about the coronal suture. The coronal suture line is incised one and one-half inches from the midline and the dura opened for ¾ of an inch and a rubber tissue drain inserted. Two such operations were done by Simmons,[28] one by F. T. Murphy and one by Green.[29] There

are no others recorded. There are two theoretical disadvantages: The opening of the suture line is apt to be accompanied by hemorrhage and injury to the brain substance, for the dura is intimately connected with the bone at this location. The drainage is not apt to be good, for the bones close together and effectually stop the opening. A subtemporal opening would seem to have the same advantages here as exist in the openings in older children. The Tweedy procedure without lifting a button of bone to ascertain what injury lies beneath is a makeshift as our Case 4 would demonstrate.

There is a large field for investigation and constructive work in the care of the newborn presenting meningeal symptoms. Before anything may be done for the stillborn child with the pulsating heart, a great deal of educational work must intervene. However, it seems that (as Meare and Taylor indicated) any method of resuscitation that notably increases the pressure in the cerebral veins, and those are the very methods that most of us use, should be discontinued. Mechanical respiratory apparatus, which we can trust to deliver air and withdraw the carbon dioxide, must be developed.

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Society Transactions

AMERICAN GYNECOLOGICAL SOCIETY. FORTY-FIFTH ANNUAL MEETING HELD IN CHICAGO, ILL., MAY 24, 25 and 26, 1920

THE PRESIDENT, DR. ROBERT L. DICKINSON, OF NEW YORK, IN THE CHAIR.

DR. EDWARD P. DAVIS, of Philadelphia, read a paper on **Analgesia and Anesthesia in Labor.**

Dr. Davis said that the commercial exploitation of so-called "twilight sleep" first drew the attention of the public to this subject. When the true value of this method was appreciated and it was relegated to its proper place in obstetrics the attention of the profession was engaged by the advocates of nitrous oxide and oxygen in labor. It was claimed that no patient should pass through labor without a thorough and painstaking effort upon the part of her medical attendant to relieve suffering and avoid exhaustion. This claim must be acknowledged just and fair, but in the effort adequately to meet it discrimination should be exercised.

That actual suffering occurs during labor and that this suffering is the same as that endured during surgical procedures accompanying wounds and lacerations or under other conditions where nerve filament are irritated, there can be no doubt. The effort to consider suffering in labor as righteous and inevitable was dissipated by Queen Victoria. It will be remembered that when Simpson proposed to give her chloroform in labor the bishops of her church forebade because pain was decreed in Scripture as the necessary accompaniment of childbirth. The Queen replied that none of the bishops had ever had a baby, but that she had passed through this experience and that her experience should decide, and so it did. She was among the first to receive the benefits of anesthesia in labor.

The best cure for the abnormalities of the first stage of labor lies in the hygiene of pregnancy. If a woman can be brought to labor with sound nerves and muscles a great gain has been made. If she can be under the care of an experienced nurse and has the natural maternal ambition and instincts, the first stage of labor may often be passed successfully without the use of drugs. The copious and thorough irrigation of the lower bowel will do something to mitigate nagging pain. The frequent emptying of the bladder, the necessary preparation for labor, moving about and occupation, will all help to pass daylight hours. The healthy woman may doze during the first stage of labor at night but very often she is unable to sleep and failure of sleep is a good preparation for exhaustion. In many cases analgesia is therefore demanded and its proper administration is of great value. It can be said that the woman will be prepared for labor in proportion as she has avoided toxemia in her pregnancy and has practiced well-regulated exercise in the open air. With this background, if sleep is denied her during the first stage, she rightly demands relief. It is evident that the merit of the so-called "twilight sleep" lay in the psychic control of the patient and in the hypodermatic use of morphine. While the former is difficult at times outside of a hospital, the hypodermatic use of morphine, preferably with atropine, is usually possible. With unruptured membranes there is little evidence that this injures the fetus nor does it seriously delay labor. In the presence of nausea a drug hypodermatically given is promptly and efficiently absorbed. It tends to promote dilatation of the cervix, and the combination is a reliable and efficient one. The proven objections to scopolamine or hyoscine are the uncertainty of action which may become exciting and its uncertain influence upon the fetus. In our experience a moderate dose of morphine and atropine has never acted unfavorably upon the child. In one case of excessive nervousness with tedious dilatation of the os and cervix in a neurotic multipara suffering with a mild bronchitis, the hypodermatic use of

codein through two nights and three days seemed to produce a condition of mild asphyxia in the child. This, however, did not prove serious and the child was readily made to breathe. In cases where patients are admitted to the hospital threatened with rupture of the uterus through impacted fetus and violent uterine contractions, the hypodermatic use of morphine may be necessary until operation can be performed. We have as yet seen no positive evidence that morphine thus used injured the child.

During the second stage of labor we believe that an opportunity should be given for the patient to establish the normal physiology of the expulsive operation of parturition. This consists in muscular contraction which after reaching the crest of its energy, is followed by a gradual cessation and relaxation, and this to be succeeded by a period of absolute rest and often sleep. If the pulse and pulse tension of the patient undergoing this phenomenon be observed, they are found to be surprisingly normal. There is no evidence of exhaustion and it is questionable whether it is well to seriously disturb this condition by anesthesia. There can be no objection to brief anesthesia at the point of greatest muscular activity, provided the conditions are such that the patient, if it were necessary, could be promptly delivered. If, however, the conditions are unfavorable for prompt delivery through the vagina, the patient may insist upon anesthesia and abandon all efforts to help herself in labor.

Pressure upon the pelvic floor is painful in proportion to the lack of development of the genital tract, the size and consistence of the presenting part, its position and presentation, and the condition of the nervous system of the patient. Unquestionably at this stage of labor anesthesia should be available and usually employed. Here those who urge the use of nitrous oxide and oxygen believe it to be a specific. Our experience, however, has not led us to subscribe to this somewhat enthusiastic claim. In our experience nitrous oxide and oxygen is not a stimulant, but is an anesthetic and asphyxiating agent. Its merit consists in its easy inhalation, its comparative safety, and the prompt recovery of the patient. It does not lessen the vigor of uterine contraction, neither does it seriously affect the fetus, nor is it a stimulus of muscular and nervous action. In some patients it produces a condition of excitement which is undesirable and it cannot be trusted if the anesthesia is to be carried to the surgical degree for forceps, version, or embryotomy. With a preliminary hypodermatic injection of morphine it may be used for abdominal section. While the apparatus necessary for its administration is comparatively simple, it is more cumbersome and complex than the can of ether and the gauze or simple inhaler necessary for etherization. While we recognize fully the value of nitrous oxygen in labor, we should not be willing to keep a patient under its influence continuously for five or six hours, neither should we be willing to depend entirely upon it in all cases of parturition.

The best quality of ether skillfully administered is successful in the majority of cases of spontaneous labor during the second stage. If given at the height of the pain, quickly removed so soon as the pain subsides, it stimulates and does not retard labor, but the moment when expulsion occurs, a few deep inhalations without air will render the patient insensible to pain, although capable of comprehending sensations of feeling, of hearing, or often of sight. The mother rouses easily after delivery and requires no anesthesia while the placenta is separating. For the insertion of stitches immediately after labor ether, with oxygen, properly administered, is comparatively safe and efficient. We have seen no evidence that such use during the stage of expulsion injures the fetus. It is true that ether is inflammable, that some patients are excited by it, that it is irritable to the bronchial tubes and kidneys, and that it is difficult to anesthetize some patients with ether, but if skillfully administered it is usually successful and its combination with oxygen renders it in our experience the safest of obstetric anesthetics. For the immediate closure of lacerations it must be remembered that all parts of the genital tract are not equally sensitive.

The comparative merits of nitrous oxide-oxygen and ether and oxygen were exemplified to the author in a recent experience. A multipara was suffering from a hepatic toxemia; while her urine remained comparatively free from albumin and casts, the ammonia percentage was rising steadily and likewise the creatine, creatinine and rest nitrogen. Vomiting was incessant and uncontrollable; the heart action increased in rapidity. As viability had been

reached it was deemed necessary to induce labor and it was suggested that to anesthetize the patient with nitrous oxide and oxygen would be useful. A careful physical examination of the patient revealed the fact that the second sound of the heart had disappeared, and that the heart was acting badly. There was no asphyxia, but cardiac dilatation seemed threatened. The anesthetizer wisely declined to use nitrous oxide and oxygen, stating that in his experience a similar patient had suddenly died during the administration of the nitrous oxide and oxygen while preparations were made to induce labor. Ether and oxygen were then given and labor was induced. After two hours of ineffectual pain with continued vomiting and bad heart action it was believed imperative to deliver the patient. Accordingly, ether and oxygen were again administered, dilatation of the cervix was completed by the gloved hand, and the child delivered alive by forceps. The action of the heart improved under the anesthetic and with the aid of vigorous stimulation hypodermatically the patient recovered from her labor. It seemed to us that in this case we were practically limited to ether and oxygen. Chloroform was prohibited by the hepatic toxemia present and also by the condition of the heart.

In the experience of the writer the combination of oxygen and ether proved remarkably successful in parturient women suffering from cardiac, respiratory or nephritic conditions which formerly demanded the use of chloroform. The writer recently had occasion to operate upon a number of pregnant tuberculous women in various stages of pulmonary tuberculosis, and employed this anesthetic with little irritation and good after-results.

For the purposes of diagnosis, nitrous oxide and oxygen may be exceedingly valuable. For thorough examination of nervous patients, for minor manipulations, such as the introduction of bag or bougies, or the introduction or removal of gauze packing, or the opening of a breast abscess, or for other possible conditions where a brief and not complete anesthesia is necessary, nitrous oxide and oxygen may be of great value. In inducing labor the writer is accustomed to employ these agents, thus enabling him to dilate the cervix somewhat with the gloved hand, to separate the membranes from the lower portion of the uterus, and to introduce from one to three bougies without resistance and suffering upon the part of the patient.

For the obstetrician analgesia or anesthesia, skillfully given, makes for more accurate diagnosis during labor and for the successful management not only of spontaneous and normal parturition, but of complicated conditions.

DISCUSSION

Dr. Carl Henry Davis, Milwaukee, Wisconsin.—Dr. Davis' paper strikes a conservative note. Dr. Davis and I agree fully in all but one particular. So far as the value of analgesia and anesthesia in obstetrics is concerned, my opinion regarding the relative merits of inhalation anesthetics in obstetrics is the same today as stated in 1917 in the three papers read that year before the Chicago Gynecological Society, the American Association of Anesthetists, and the American Medical Association.* My conclusions may be found in those three papers as published.

My early experience in obstetrical anesthesia and analgesia was largely with chloroform and ether. Nitrous oxide-oxygen was rarely used except when other anesthetic agents were contraindicated. I have used no chloroform since 1909. I have tried ether in every manner suggested. During the past few years I have used nitrous oxide-oxygen analgesia as a routine in obstetrics. The results with ether, whether given by drop method or vaporized with air or oxygen, were in no way comparable with those usually obtained with the nitrous oxide-oxygen analgesia. With Dr. Davis I agree that nitrous oxide-oxygen should not be given continually for a long period. I first called attention to the possible dangers of the long-continued analgesia in 1917. But given intermittently, it has a distinct value over ether in most cases because it is quickly absorbed and quickly eliminated. Ether is very slowly eliminated. It has cumulative effects on mother and child. It weakens the uterine contractions and frequently causes nausea and vomiting. When the first few whiffs of ether are given to an obstetrical patient we substitute something which may be worse to her than pain, but

*Am. Jour. Obst., 1917, lxxvi, No. 4; Am. Jour. Surg., 1917, xxi, Anesth. Suppl., p. 98; Surg., Gynec. and Obst., 1918, xxvi, 170.

given at the culmination of the contraction it usually surprises and quiets the woman. After a few inhalations she gets the cumulative effect and a definite relief of pain.

Ether, unlike chloroform, stimulates both respiration and circulation. A patient taking ether with air never has cyanosis from the ether and, since it does not reduce the amount of oxygen in the circulating blood, I fail to understand why ether given with oxygen can be so much safer in cases of toxemia than ether given with air. Oxygen with ether does not change the physiology or lessen the irritation to the kidneys. The safety of both ether and chloroform may be increased by the administration of alkalies before delivery and after.

Recently I have been using nitrous oxide-oxygen analgesia intermittently for mid- and low forceps deliveries as well as in normal labor. The nitrous oxide-oxygen is given to a deep analgesia or light anesthesia while the forceps are applied. The mask is then removed and thereafter the gas administered intermittently as in normal labor. The patient is instructed to bear down during contractions while gentle traction is made on the forceps. For primiparæ I do a primary posterior episiotomy. There is usually an easy delivery with a minimum of traction pressure on the head. I cannot accomplish this cooperative type of delivery with ether regardless of its administration. In my experience nitrous oxide-oxygen has proved to be the most satisfactory inhalation analgesic for both normal and operative obstetrics. If a long continued anesthesia is required, I use ether.

Dr. W. Francis B. Wakefield, San Francisco, California.—I have striven for the last five or six years in San Francisco to relieve women of the pain of childbirth. I have used scopolamine anesthesia in all my private patients and up to the present these number 700 cases. I have no clinical practice in obstetrics, so that scopolamine is used entirely on the higher class of women of the city. I have found scopolamine as an anesthetic an absolutely perfect therapeutic agent and it is discouraging to a man who has used it as honestly and as faithfully as I have done with such gratifying results, to have men like Dr. Davis, Dr. Williams, and others relegate this thing to the background as a perfectly useless therapeutic agent.

I do not know why it is that the use of scopolamine has been unsuccessful in the hands of so many men. I believe it is because of bad preparations of the drug that have been used. Scopolamine, except in fresh solutions from the original crystals that have been carefully preserved from disintegration, or in the form of ampules, is useless.

In tablet form I consider scopolamine almost useless as an anesthetic and often injurious, because exposure of the scopolamine crystals to air will injure its therapeutic action and produce a wild delirium instead of a somnolent effect.

There is nothing psychic about my use of scopolamine. The patient is under more or less profound anesthesia from the time labor pains become regular until the child is born. No other anesthetic is used at all in labor except during the last half or dozen pains, when nitrous oxide and oxygen are used in conjunction. My patients go through labor without any reaction, without any pain whatever, and I consider a therapeutic agent that will produce this result without harm to the mother or child worthy of consideration and should not be carelessly relegated to the background.

The *San Francisco Examiner* not very long ago published an article in which Dr. Williams was credited with saying that it (scopolamine) had been tried and found wanting. Now, that may be perfectly true of Dr. Williams' experience with it, still it gives a bad impression to the general public and undoes very largely the work of others who have had great success with the use of this drug. Scopolamine, properly used, with proper preparation of the drug, is a valuable therapeutic agent.

Dr. Charles E. Paddock, Chicago (by invitation).—I would like to ask Dr. Wakefield some questions. He states that he uses scopolamine-morphine. He tells us that he has had 700 cases. Does he mean that he has used it in every case? When does he begin the use of it, at what stage in labor, and what are the dosages that he gives? Does he give it regardless of pain and has he had any asphyxiated babies? Can he use scopolamine-morphine without an additional anesthetic?

Dr. John O. Polak, Brooklyn, New York.—As usual, Dr. Davis has presented a conservative paper, but I do not think it is fair for him to condemn morphine and scopolamine, though his caution regarding the indiscriminate use of these anesthetics is well taken.

There is no doubt in the minds of all obstetricians that anesthesia in labor is a necessary adjunct. In order to give our patients the rest of labor it is necessary to relieve some of their suffering,—particularly is this true in the nervous type of women with whom most of us are dealing. We can do this better with a combination of scopolamine and morphine than with morphine and atropine. The effect depends on how we use it and when we use it. In our clinic it is a routine first stage procedure. We do not try to get amnesia except in a limited number of instances. But we get some degree of analgesia and the analgesia holds over so that these patients, during the dilatation stage, enjoy the full action of their uterine muscles and can complete the second stage mechanism which Dr. Davis says is so necessary. When the fetal head reaches the pelvic floor and during the perineal stage anesthesia is again necessary, whether the delivery be accomplished by episiotomy, prophylactic forceps, or by nature. During this period we have found after a considerable experience with nitrous oxide-oxygen, that ether with oxygen is the safest and best anesthetic.

One point in closing is, that we discontinue the anesthetic after the delivery of the child and allow the patient to come out from her anesthesia before any reparative work is done. This allows the uterus to contract and minimizes the bleeding. When retraction is firm anesthesia is continued for the repair of pelvic injuries.

We have not had any postpartum hemorrhages since we divorced the anesthesia of labor from the anesthesia of repair. I trust I make myself clear on this point.

Dr. Joseph L. Baer, Chicago (by invitation).—I should like to subscribe almost without reservation to the views of the essayist.

My first experience with nitrous oxide anesthesia dates back to 1904, at which time, as a member of the house staff of the Michael Reese Hospital, I was sent to Wesley Hospital to watch Dr. Weller Van Hook do perineal prostatectomies under nitrous oxide. We then started to use it at the Michael Reese Hospital and for several years I was official anesthetist, during which time I gave many hundreds of nitrous oxide anesthesias for abdominal surgery.

In obstetrics I believe the only place for nitrous oxide-oxygen is in the second stage of labor before the end, and only with such women as cannot be controlled by the presence of the accoucheur who is cooperating with his patient during her second stage contractions. My view is, if the accoucheur is with his patient and steadies her, so to speak, she will be very much better and will usually choose to do without anesthesia during the second stage until near the end, at which time I rather prefer ether to nitrous oxide. The patient is usually controllable under light ether anesthesia for the purpose of actual delivery and is not always controllable under the nitrous oxide-oxygen.

During the first stage I use almost entirely, if I use any combinations, morphine hypodermically and chloral hydrate by mouth or rectum.

A word as to scopolamine. When "twilight sleep" swept over the country we at the Michael Reese Hospital decided to give it a thorough trial. My article published in 1915 dealt with a series of 70 cases. It was for us a conclusive answer and we have discarded its use in obstetrics.

Dr. Benjamin P. Watson, Toronto, Canada.—I think one of the very important things which Dr. Davis told us in his paper was that we should be particularly careful of the patient during her pregnancy. If one can see a patient frequently during the course of pregnancy and gain her confidence and be with her for some time at the commencement of labor, one can do a great deal to help that patient in her labor and stave off the administration of any anesthetic for some time at any rate. On the other hand, there are patients to whom one must administer some form of anesthetic in the first stages of labor.

I am entirely in accord with those speakers who have upheld the use of scopolamine and morphine. We use it frequently, but never before giving the patient a chance to go through the first stage without an anesthetic. I think it is a mistake to administer drugs as a routine when a certain stage of dilatation of the cervix has been reached, but if the patient is getting worn out, is fretful, and not bearing pain well, morphine and scopolamine or the hydrobromate of hyoscine are valuable drugs. It has been my experience that with a preliminary dose of 1/6 gr. of morphine and 1/150 gr. hyoscine hydrobromate there is no bad effect on the mother or child. The morphine is not repeated and if the hyoscine hydrobro-

mate is repeated, it should be given in from 1/300 to 1/400 gr. dose. With that administration we usually have to give some general anesthetic toward the end of the second stage. We have used both nitrous oxide and oxygen and ether, and personally I trust to the administration of ether toward the end of the second stage of labor. I have seen no bad effects from morphine-hyoscine administered in the manner I have mentioned.

DR. RALPH H. POMEROY, BROOKLYN, NEW YORK.—It seems to me, this society must recognize from the nature of the paper and the discussions that we are not dealing with a fixed and standard process of drug administration for the relief of the distress of labor.

In order to meet the psychology of satisfaction on the part of the patient, the obstetrician must be convinced that when she is through with her labor "she has not had a terrible experience." If we meet that requirement at all, we must recognize the fact that there is no fixed order of the administration of these analgesic and anesthetic drugs which can be standardized. Furthermore, we must understand that labors cannot be standardized and that we must have equipment, knowledge, and experience to apply to the variations of labor, the variations of temperament, and the adaptation of our knowledge and equipment to the psychology of satisfaction for that patient. Any organization or hospital equipment, or any teaching staff that does not recognize the usefulness of various combinations of morphine, scopolamine, nitrous oxide gas, ether, chloroform, worked out with reference to the individual case, fails to appreciate the importance of this subject.

DR. E. P. DAVIS (closing the discussion).—May I suggest to Dr. Carl H. Davis that in his last statement relative to analgesia and nitrous oxide-oxygen in low forceps he introduced an entirely different topic, which will be discussed by Dr. De Lee in a later paper.

Have we come to the point in obstetric science where natural spontaneous labor is not to be expected and should we substitute an artificial, more or less surgical, procedure for every parturition? If so, then nitrous oxide-oxygen anesthesia and low forceps is a surgical procedure, and the anesthesia is only a small part of it, and Dr. Davis has gone a little further than my paper went. I have nothing to say on that point except to bring up the question, are we going now to consider sharply that position?

As regards scopolamine which is used by our member from San Francisco (Dr. Wakefield), I was very much interested in hearing his experience. Most of us are not so situated that we can obtain absolutely pure drugs in this part of the world, and the importance of furnishing absolutely pure drugs was shown by the German government and the German manufacturers who instituted propaganda during the flowery days of scopolamine to give it by the mouth.

After hearing the discussion it is evident that there is a psychologic interest exercised by the gentleman upon his patients, which I envy him.

Regarding the remarks of Dr. Polak, it is always interesting to have him come out with something which seems good to him. It strengthens me. I think he is right in what he has said about labor and repair. They are not the same thing and hence anesthesia should be different.

Dr. Baer in his conservatism comforts me also, and I am glad to find that even in the stimulating atmosphere of Canada, Dr. Watson finds ether is a good anesthetic for women in parturition.

As regards Dr. Pomeroy's remarks, he touched on a very important thing. After all a woman in labor is approaching a supreme physiologic test and the damage to her being is psychologic. She is at present the psychologic entity of modern civilization. She cannot be considered *en masse*. There is no standardization, and hence we must approach the patient with the idea of helping her the best we can.

I leave seriously with you this one thing. I did not see that patient who died just as labor was being induced under nitrous oxide-oxygen. It was not my case. This woman had been given stimulation. She had been vomiting for some time. She was prepared for the induction of labor by stimulating her heart. It is the experience of anesthetists that a stimulated heart yields rapidly and fatally to nitrous oxide-oxygen. I may warn you therefore that if you attempt to anesthetize patients with nitrous oxide-oxygen who have been toxic and to whom it is necessary to give stimulation, you may have collapse. She died under inhalation of the gas without the uterus being touched. It was such a warning that I have taken the liberty of bringing it before you.

DR. BENJAMIN P. WATSON, of Toronto, Canada, read a paper entitled **Induction of Labor; Indications and Methods, with Special Reference to the Use of Pituitary Extract.**

Dr. Watson said: The communication is based upon 150 cases from the public wards of the Toronto General Hospital under my care and from my private practice. In these, labor was induced on the following indications:

Pregnancy toxemia	38
Pregnancy prolonged beyond calculated term	65
Small pelvis	25
Large size of head	10
Pulmonary tuberculosis	3
Dead fetus	3
Heart disease	1
Glycosuria	1
Anemia	1
Placenta previa	2
Hydramnion	1
Total	150

The fact that a patient has gone beyond the calculated date is not evidence that the child is postmature, and is not in itself an indication for the induction of labor. In arriving at the average we are dealing with cases with a possible variation of three weeks and every now and again we are bound to have cases at either extreme. It is, nevertheless, our duty to watch carefully all patients who go over the calculated term and to examine for the purpose of ascertaining the size of the fetal head in relation to the pelvic brim. At the first indication of any disproportion labor ought to be induced. This will be indicated in a primipara by nonengagement of the head or, by a head previously deeply engaged in the brim tending to become more mobile; and in a multipara by difficulty in pushing the head down into the brim. Multiparæ who have previously had large children and hard labor should not be allowed to go beyond the calculated date.

Small Pelvis.—Pelvimetry ought to be routine in obstetrical practice. Every woman who engages a doctor for her confinement is entitled to expect that he will satisfy himself by actual measurement that she has a pelvis of an average size. This measurement ought to be made by the seventh month, so that if the pelvis be found to be small the advisability of the induction of premature labor may be considered.

Pregnancy Toxemia.—Systematic urine examinations and estimation of blood pressure afford early opportunity for the detection of pregnancy toxemia and when discovered early it is usually easy to treat. In a small proportion of cases improvement does not result and in them induction of labor may be indicated in the hope of averting an actual eclampsia. The indications of immediate danger are a high and rising blood pressure (140 or over), small excretion of albumin laden urine, the persistence of headache or failing vision. Persistence of all or any of these in spite of appropriate treatment, calls for the termination of the pregnancy.

Labor has been induced in these 150 cases by bougies, bag, quinine, pituitary extract, and quinine and pituitary extract. As the indications for the induction of labor have become more numerous of late years, and the wisdom of it has become apparent in a large number of cases, so we have felt that nonmechanical means which expose the patient to no added risk from manipulation and sepsis ought to be employed if they are in any way effective. There are two drugs which have a marked effect in stimulating uterine contraction, and stimulating it in a manner which very nearly approaches the intermittent activity characteristic of normal labor. Those two drugs are quinine and pituitary extract. This action of quinine has been known for a very long time and it has been used in small doses for the purpose of stimulating uterine contraction during labor. Of late years it has also been extensively employed in large doses for the purpose of inducing labor. The results obtained

have been somewhat varied. This has depended upon the time in pregnancy at which it was sought to induce the labor and upon the mode of administration. We have found that much better results are obtained when the quinine is given in solution than when it is given in the powder form. We employ the hydrochloride of quinine and administer along with each 10 grains, 10 minims of dilute hydrochloric acid. This seems to increase the solubility and the rapidity of absorption. In the ordinary routine the patient first takes 1 ounce of castor oil, this is followed in two hours by 10 grains of quinine in the above solution. This dose is repeated at two hour intervals until 30 grains are taken.

The use of pituitary extract in obstetric practice is of much more recent date. In 1913 I published a paper reviewing the literature, and there pointed out that Blair Bell in 1909 was the first to employ in practice the results of experimental investigations carried out with the extract of the pituitary gland up to that time. Since then a great mass of literature has accumulated on the subject. It is universally recognized that it is a most valuable agent for accelerating the second stage of labor when delay is due to feeble uterine contraction. In most of the articles which have appeared the reader is warned against using it for the induction of labor or before the cervix is fully dilated. We have used it extensively for the induction of labor and during all stages of labor and have never had any bad results. In 1913 I recorded three cases in which I had successfully induced labor by this means. One of those was at the eighth month, one at full term, and one at three weeks *post* term. I stated then that the method was worth an extended trial and I think that my later results bear this out. The procedure used is to begin with a dose of ½ to 1 c.c. administered intramuscularly with a long needle. In most cases uterine contractions commence in about ten minutes and increase in severity during the next twenty minutes. At the end of this time the second injection of ½ c.c. is given. If after a time the contractions tend to weaken, or to come at longer intervals, the dose is repeated. As many as six or eight doses may thus be given at intervals of about one-half hour. The important point is to administer a further dose before the effects of the previous one have entirely passed off. The effects from a single dose appear only to last for about one-half hour and there is no cumulative effect. Sufficient doses must therefore be given to keep up uterine contractions sufficient to produce a certain amount of opening up of the cervix. When the cervix has begun to open and the membranes to bulge into it the uterine contractions will continue without the further administration of the drug. The failures which we had in the beginning were the result of not pushing the dosage far enough. We have found it perfectly safe to give 8 or 10 ½ c.c. doses at half-hour intervals.

Rather a curious point comes out here, that in 19 multiparæ the average was 19 hours, as contrasted with 18 hours as an average in 18 primiparæ.

With the bag method the average time elapsing before labor began was 13 hours and the average duration of labor was 10 hours. This number, of course, is very small and not sufficient to draw conclusions from.

With quinine alone, in 25 cases the average time elapsing from the last dose to the definite onset of labor was 7 hours, while the average duration of labor was 9 hours, 7 for multiparæ and 11 for primiparæ.

With pituitrin alone in a total of 18 cases the average time elapsing between the first dose and the definite onset of labor was 2 hours. The average duration of labor was 10 hours, 16 hours for 5 primiparæ and 9 hours for 13 multiparæ.

With quinine and pituitary extract, in a total of 62 cases, 53 were successful; 9 were totally unsuccessful. Six of the successful cases required repetition of the routine before labor began. In the 53 successful cases the average time elapsing between the first dose of pituitrin and the onset of labor was 2 hours and the average duration of labor 10 hours; 14 hours for 23 primiparæ and 7 hours for 30 multiparæ.

Recently two of my colleagues, members of my staff, J. G. Gallie and W. A. Scott, have recorded a series of cases (65) in which they used pituitary extract alone for the induction of labor. Out of this total 55 were entirely successful. The average number of doses given was 3, the average duration of labor was 7 hours. There was one fetal death 28 hours after delivery from atelectasis. The delivery was normal in 45, by forceps in 9, and by version in 1.

Taking these results along with the ones which I have recorded, I think we must recognize that the method has a definite place in obstetric practice and should be considered before other methods are adopted. It has very special advantages in cases of slight disparity between the head and pelvis, as it does not in any way prejudice the Cesarean operation should it prove to be necessary; an argument which cannot apply to the bougie or bag method.

DR. CHARLES B. REED, of Chicago, Ill., read by invitation, a paper entitled **Induction of Labor at Term.** (For original article see page 24.)

DISCUSSION ON PAPERS OF DRS. WATSON AND REED

DR. RALPH H. POMEROY, BROOKLYN, NEW YORK.—I cannot personally accept the general proposition that induction of labor slightly before, at, or after maturity, adds in any important way to the safety of the mother or child. I believe there is added a certain amount of danger to the mother, especially so in primiparæ, if induction is carried out by foreign bodies introduced into the uterus.

The second paper gave me no enlightenment as to a point from which to start discussion, owing to the fact that it made no distinction between the statistics in multiparæ and primiparæ. I have no more knowledge than the rest of you as to exactly why or when labor begins. I do know two factors that are necessary for the progress of labor after it has begun; one, that the ovum must be detached or easily detachable in whole or in part, and second, that the uterus must make intermittent contractions resulting in retraction adapted to a diminishing capacity of content. Unless the ovum is detached it cannot descend. If the uterine content cannot descend there develops with persistent labor some degree of spastic retraction of the uterus instead of progressive retraction of that organ. I abandoned the idea many years ago that you can induce labor necessarily by dilating the cervix when I was experimenting with dilating bags in the endeavor to shorten the first stage. I found by many experiments that forced dilatation of the cervix, notably as in multiparæ, did not necessarily induce labor.

For several years past I have been trying to manage all labors without using induction at all.

If we are logical and correct in our belief that there is real danger in making vaginal examinations in labor in the first stage, if we accept the proposition that rectal touch is of real value in cutting down the dangers of contact infection carried from the vulva up to the cervix or cervical glands, if we admit that vaginal examinations add something to the risk of infection, we must admit also that we should try to manage labors without putting a foreign body in the cervix, which, in remaining there obstructs drainage, and causes traumatism and contusion. One can induce labor in a healthy multipara, who has no real disproportion, with comparative safety and certainty by merely rupturing the membranes. If I must put a foreign body in the uterus, I prefer to use a bougie of some form, because it can be managed so it does not block drainage from the cervix.

I hope that Dr. Watson has discovered a process by which we can safely and promptly induce labor without cervix contact in the few cases in which we wish to empty the uterus forthwith. But at the present time, if the call is for an empty uterus at once, I prefer to cut below if the child is nonviable and above if it is viable.

DR. GEORGE W. KOSMAK, NEW YORK CITY.—A great deal of very valuable material is presented in the two papers of Dr. Watson and Dr. Reed and I do not feel like taking the pessimistic attitude on this subject of my predecessor in the discussion. It seems to me, as we develop our experience, we are guided more or less in what we do for our patients by what that experience has taught us. I have for a number of years carefully watched my patients with the idea that if they went on to what was accepted to be the normal end of their pregnancy, labor be induced. Nothing that has been said against this attitude has convinced me that I have been wrong. I feel like agreeing with Dr. Reed that it is a procedure we must always bear in mind but apply it to our patients individually rather than as

a routine procedure. I do not want to be understood as advocating the induction of labor at term as a routine, but every one of our patients should be watched, particularly primiparæ, and if we have had the opportunity of making repeated antepartum examinations, I think we can feel rather certain as to when the child is large enough. In my own experience it has never happened in any case which I had under observation that a premature baby resulted from this method.

I do not feel I can quite agree with Dr. Reed in his statement of absolute measurements. It is not really so important whether a child measures forty-eight or fifty-two centimeters, or whether certain diameters of the head are this or that. It is more important to study the type of patient with whom we are dealing. Some women can deliver themselves without difficulty of babies weighing nine and a half or ten pounds. Remember, we are dealing here with normal pelves. Others have difficulty in delivering themselves of a six pound child. In each instance careful pelvic measurements have shown we were not dealing with any abnormality. I think it is the latter type of patient that must be carefully observed, and if we have conducted such observations, we ought to be ready to interfere when we think the proper time is at hand.

Another problem relates to the induction of labor in cases of toxemia. We ought to base our attitude in these cases, not on the attempt to bring the child as near term as possible, but at such a period when a viable child can be secured. I think the attempt to carry a toxic woman through the last three or four weeks of pregnancy is a very great mistake because the baby is not in a satisfactory condition during this period and damage results to the maternal organs that can never be corrected.

I was much interested in what Dr. Watson said about the use of quinine in inducing labor, because I only recently read the reports of our colleagues in South America who live in malarial districts and who state that the employment of large doses of quinine for the treatment of malaria in pregnant women has been without result in so far as the induction of labor is concerned. There must be some difference between the women of Canada and South America. Perhaps Dr. Franklin Martin, who has recently visited South America, can explain it.

I was also interested in Dr. Watson's remarks about the use of pituitrin. It seems to me, that he employs very large doses for this purpose. I am sure the New York atmosphere would not permit with safety the continuous employment of doses of half a c.c. as many times as he recommends.

In conclusion, I want to refer again to the valuable work which has been done in the last few years by Dr. Reed and others in bringing to our attention the necessity of watching each individual case of pregnancy to determine as closely as we possibly can whether that particular fetus is going to be able to go through that particular birth canal, and we think the time is at hand not to hesitate to help that woman out of possible future difficulty by inducing labor by methods that have now been pretty thoroughly worked out and demonstrated as safe.

Dr. Hugo Ehrenfest, St. Louis, Missouri.—I would like to add a few words to what Dr. Kosmak has said in relation to the use of pituitrin. I believe it is safe and necessary to continue the old teaching that pituitrin should not be used in any other stage of labor except the second or third or after the third.

In my experience considerable difficulty arises from the dosage question. Dr. Kosmak says that one-half c.c. is too large a dose. I use smaller doses. Then again the difficulty in my experience is that pituitrin preparations vary. I have for years limited myself to one preparation because I feel I know what I am using. If we use a different preparation we have absolutely no idea what dosage we really use. I find that when a patient receives pituitrin for the purpose of starting labor, there is a decided difference whether the patient after the injection remains lying on her back or whether she gets up and walks around. I have no doubt in my mind that when a patient is kept lying on the back the contractions continue much longer. Why, I do not know. I would like information from Dr. Watson on that point.

Regarding the blood pressure which was mentioned by Dr. Watson, I should like to emphasize that the special effect of various preparations seems entirely dependent upon the amount of histamine it contains. Some preparations raise the blood pressure very little, others very much.

With reference to the effectiveness of starting labor that way, we all probably realize the pronounced difference between primagravida and multipara and between labor starting before normal term and after that. There is no doubt a multipara who has gone over time responds most promptly.

I desire to emphasize one point already made by Dr. Watson, that such a method of starting labor has great advantages over all mechanical methods, if one should be forced to resort to Cesarean section afterward.

I should like to call the attention of Dr. Watson to the new literature on the intravenous use as an oxytocic, of the bi-hyperchlorate of quinine.

Dr. J. Whitridge Williams.—Two things interested me very much in this discussion. The first is Dr. Watson's contribution concerning the effect of pituitrin, and I earnestly hope that the future will bear out his experience, for if it does so he will have added a great boon to workers in obstetrics.

The other thing which interested me very much was what Dr. Reed has said concerning the induction of abortion in post-mature children. In my own work, I have recognized practically only two main indications for the induction of labor. First, in women suffering from the toxemias of pregnancy which do not yield to prophylactic treatment. In such cases we interfere primarily in the interests of the mother and only secondarily for the sake of the child. The other indication is found in patients who have reached term, and in whom the child promises to exceed the normal limits. On the other hand, I have never been a friend of the induction of premature labor in contracted pelves, and in my whole experience have only employed it on several occasions.

During the past week I have had an experience which pretty well exemplifies my practice. I did a Cesarean section at an appointed time before the onset of labor upon a woman with a normal pelvis, solely on account of the size of the child. The sequel showed that my diagnosis was correct as the child weighed 4300 grams and had a biparietal diameter of 11 cm. In other words, definite disproportion existed. At the same time there was a colored woman in the service with a generally contracted rachitic pelvis with a diagonal conjugate of 9 cm. She had a small child and no disproportion, was left alone and had a spontaneous labor.

At term I think that we should be extremely careful about inducing labor, and should not think of resorting to it unless we have a perfectly justifiable indication. To my mind such an indication does not consist in the mere fact that so many days have elapsed since the last menstrual period, or that the child has attained the average normal size, but we must demand that it has attained a size which threatens to give rise to disproportion and will certainly do so if allowed to become still larger. In other words, I see no reason why labor should be induced in a woman with a normal pelvis unless the child definitely exceeds the usual size, as to do so would simply lead to what I am afraid we are approaching in so many lines, namely, excessive operative interference. I am sorry to say that this tendency has already become established in regard to Cesarean section and, unless the indications for the induction of labor at term are carefully guarded, that operation will likewise be soon abused to a similar extent.

However, I do not wish to be misunderstood in this matter, as I regard with suspicion every woman who has reached the calculated date of confinement without falling into labor. In such cases it is my practice to palpate the patient each week and, as soon as it appears that the child threatens to exceed the normal size, labor should be induced. In general I have found the most satisfactory method of inducing labor, where haste is not essential, is by the use of a large Wales' bougie. I use one the size of the finger and it is so satisfactory that I employ bags practically only in cases of placenta previa, which are not under discussion.

Dr. Harold C. Bailey, New York City.—On our Bellevue Hospital service we have been committed to the idea of the induction of labor at term, but I cannot say that it has been a routine. Various factors have prevented this. The service is so busy from admissions that it is difficult to do a procedure like this regularly; but every week we go over the

antepartum cases and those who seem to be oversize or full term have their labors induced. We always start by a method somewhat similar to what Dr. Watson has outlined. We dilate the cervix to two fingers, and then give a dose of castor oil directly, and 10 grains of quinine some two hours afterward.

A great number of these women will fall into labor in the early morning hours. Since the first of the year, in addition to this, we have been adding 3 minim doses of pituitrin, giving four doses one-half hour apart.

The records kept of patients at Bellevue have been of the dispensary class and I can give the records only from my own notebook. It shows there were 35 cases in which the induction of labor was attempted by means of dilatation to two fingers, then the giving of castor oil and 10 grains of quinine. Of these 35 cases, 12 were primiparæ, and only 3 or 25 per cent went into labor the first twenty-four hours. Of the remaining 23 multiparæ, 56 per cent of them went into labor the first 24 hours. Taking the combined 35 cases, 45 per cent went into labor without any further treatemnt. If the woman does not go into labor following this procedure we usually allow 48 hours to go by and then a No. 4 Voorhees bag is inserted. We do not use a bag less than No. 4 except in placenta previa. If we talk from the standpoint of the treatment by bags and if we take the records from the Manhattan Hospital service where induction of labor at term is not routine, we find in going over 161 cases of induction, there are 60 cases of vertex presentations induced for rupture of the membranes or dry first stage, or oversize of the fetus. Of these 60 cases which might be termed more or less normal, there was a stillbirth rate of 3 cases, but one case was an anencephalic child. Disregarding the monstrosity we still have stillbirths at 3.3 per cent and the mothers remained normal.

Dr. Rudolph W. Holmes, Chicago.—One of the mysteries of life will be unfolded when scientific knowledge tells us when pregnancy begins. Until that time comes, we always will be uncertain as to what full time or term means. The uncertainty of the onset of pregnancy carries with it a doubt as to maturity measured by days or weeks. There are large men, women and children—just as there are small ones. Children, for example, of ten or twelve years of age, may be over large or very diminutive for their years, yet at maturity are just ordinary individuals. Unborn babies vary as to size, yet are normal for the period of gestation considered. They may be large or small as the case may be, yet are "normal." We all know that there is a material variation in size of children born from different mothers. Size, *per se*, is a poor criterion of what we call maturity, or, for that matter, frequently of viability. I have had one woman who gave birth, on the day of reckoning, to a thirteen and a half pound baby. No one would hold that child to be postmature. Some years ago I saw a woman who was about to have an induction because the fetus was over large, yet, at my insistence she went on, and nearly three months later gave birth spontaneously to her baby. In this latter case an erroneous miscalculation of the period of pregnancy was made, and such mistakes may be readily made. We all have seen babies born prematurely, by the accumulated evidence of all data, yet had the appearance and viability of full term babies.

With reference to pituitrin, in my belief it is the most powerful drug known. It has been stated by an official of one of the large pharmaceutical houses that the fluid contained in the cubic centimeter ampule contains, as nearly as they can compute the dosage, one three to four millionth of a grain of the active secretory substance of the pituitary body. Such an active substance demands the most circumspect exhibition. The pituitary substances are used most promiscuously by the profession and in a very haphazard manner. I have long felt that the fact that so few catastrophies have followed this indiscriminate use is dependent upon the fact that there is such a wide difference in the potency of the products of different manufacturers. Dr. Young, of the Public Health Service, investigated the potency of various pituitary fluids and found that some were as low as one-eighth the strength of some others. Over a generation ago, it was appreciated that the indiscriminate use of ergot, given in labor, was followed by most lamentable catastrophies: I believe we are arriving at the same realization of the dangers of pituitary substance. I am convinced that the exhibition of a full ampule of pituitary fluid, yes, even one half ampule doses, repeated half hourly for six doses as a means of inducing labor will sooner or later spell disaster, especially if employed by physicians who are unable to gauge its dangers.

DR. N. SPROAT HEANEY, CHICAGO.—Since there are so many involved points in the discussion of the indications for the induction of labor, I shall confine my remarks entirely to that technic. The case that Dr. Holmes spoke of where a patient went three and a half months after she was estimated to be at term, as well as the case of a certain Lilliputian baby will illustrate what I mean when I say "involved." In reference to these particular cases, however, I would say that where such cases happen, the "specialist" had better go into eye, ear, nose and throat rather than into obstetrics.

At the Presbyterian Hospital we formerly used bags to induce labor almost to the exclusion of other methods, until we became convinced we were having more trouble with the bags than benefit gained by their use. Latterly we have used quinine and castor oil almost exclusively. At the time of examining the patient to see whether labor should be induced or not, we separate the membranes widely from the cervix, then give castor oil and quinine. We always give small doses at intervals, since we find an occasional patient who has an idiosyncrasy to quinine. Even three grains will produce an annoying disturbance occasionally, in which event we may stop the quinine before we have poisoned our patient too extensively. In the event that the first administration of oil and quinine is not successful, we allow the patient to rest before repeating the dosage. During the last two years following this method I have resorted to the bag only three times in the induction of labor. During this period of two years we have had only one baby born dead. These cases were not only induced cases but cases also that went into labor spontaneously. The death in this case was caused from the separation of the placenta. In a second case of induced labor where the placenta separated we did a hasty forceps, and though the child was born alive, it died soon after from forceps injury.

DR. BENJAMIN P. WATSON, TORONTO, CANADA (closing the discussion on his part).—In regard to the dosage of pituitary extract, I myself was rather afraid of it when we first began to use it, but I must say that I am no longer so. The thing to remember is that the effect passes off very quickly. If there is any effect from one single dose, it passes off inside of twenty minutes or half an hour. As there is practically no cumulative effect of the drug, I do not see that it makes any difference how often you repeat it, provided you allow an interval of twenty minutes or half an hour. You must repeat the dose when the effects of the previous dose pass off. I have never seen any bad effects from the use of the drug given in that way.

There can be no question that there is a great difference in the different preparations of pituitary extract. A reliable preparation should be used.

I have not seen any difference in the action, whether the patient was allowed to walk about or kept in bed. We usually keep them in bed and do not allow them to walk about.

There is a difference, of course, in the results one gets with primiparæ and multiparæ. The results are less certain in the former. The time of pregnancy at which labor is induced has an influence. If the cervix is not taken up at all the induction is more difficult because labor does not begin until the cervix is sufficiently dilated and there is sufficient detachment of the membranes to produce a distinct bulging so that the ovum may descend.

As regards quinine, the method of administering it in acid solution is an important one. This was demonstrated in our use of quinine at Salonica. We took quinine salphate in powder 10 grains a day without any discomfort. When taken in acid solution one experienced a buzzing in the ears, showing that the quinine was acting efficiently. I have given my patients quinine in this form for the induction of labor. In the same way intravenously a much more rapid action is obtained, but I have never attempted to give the drug intravenously in these induction cases.

DR. CHARLES B. REED, CHICAGO (closing the discussion).—There is little to add. It has been quite gratifying to note the kindliness with which the bag method of induction has been received, a kindliness which arises, no doubt, in large part from a polite self-restraint and is on that account the more appreciated.

I am glad that Dr. Holmes has reported in this connection his case of prematurity in which induction of labor was urged. It is in just such badly studied cases that we realize the necessity of making, as a routine, those extra-uterine measurements of the intra-uterine child which I have emphasized in my paper.

DR. JOSEPH B. DELEE, of Chicago, read a paper entitled **The Prophylactic Forceps Operation.** (For original article see page 34.)

DISCUSSION

DR. J. WHITRIDGE WILLIAMS, BALTIMORE.—I am sorry to say that there are only two things in Dr. DeLee's paper with which I entirely agree. The first is to allow the cervix to undergo spontaneous dilatation, and the second is the correctness of the general anatomical considerations which he has adduced. With the rest of it I do not agree. Doubtless Dr. DeLee, or the majority of those present can deliver women in the manner he has described and leave them in better condition than had they been delivered in the usual way by the average practitioner. On the other hand, I believe that if his practice were to become general and widely adopted, women would be worse off eventually than had their labors been conducted by midwives.

Recently I have inaugurated the practice of having all my patients come back to the hospital for reexamination one year after delivery in order to determine how they have stood the strain of labor, and what harm delivery has done them. In many instances, I have been greatly surprised to find that they are in far better shape than one has any right to anticipate, but on the other hand I am occasionally surprised to find how marked a degree of relaxation may follow an easy spontaneous primiparous labor. Furthermore, I am quite convinced that if Dr. DeLee's practice should become general, and the women were examined in a similar manner that they would be found to be worse off than the majority I see.

Passing on to what Dr. DeLee has said concerning the treatment of the third stage of labor, I do not hesitate to state that I think he has been perniciously active. In the great majority of cases it is unnecessary to introduce the hand into the vagina, as in most patients the fundus will be seen to rise up within ten minutes after the extrusion of the child, which indicates that the placenta has become separated from its area of attachment and lies in the distended lower uterine segment, while all that is necessary for its extrusion is the application of slight pressure above by the obstetrician.

If I have understood Dr. DeLee correctly, it seems to me that he interferes 19 times too often out of 20. Of course what I say applies to normal labors, but cases of hemorrhage must be treated in a totally different manner. I therefore believe should his recommendation be generally adopted that it would do an immense amount of harm and far counterbalance the good which it may accomplish in his expert hands.

PRESIDENT DICKINSON.—We have the great honor of having with us a distinguished English obstetrician, Dr. Eden, of London, whom I will ask to discuss this paper.

DR. THOMAS WATTS EDEN, LONDON, ENGLAND.—I doubt very much whether this is a prophylactic procedure that Dr. DeLee has described to us. He says he is going to prevent something. Unless he prevents something we are in fear of, I do not think he has made out a case for his operation. He says he prevents laceration which will result in prolapse. He has made two or three admissions on the subject, however, which I think we should not overlook. In the first place, he says he cannot prevent injury which occurs to the pericervical fascia. We do not know it by that name in England. I take it he meant the pubococcygeal fascia. He admits he cannot prevent injury of that which occurs from dilatation of the cervix. It is the injury to that fascia which is the cause of cystocele and his operation is claimed to prevent cystocele. He does the operation to prevent diastasis of the levator ani muscles and fascia which covers them. The time at which he does this is when the head reaches the pelvic floor, and I take it he cannot do the operation when the head presents at the vulva. At the moment it is lying between the levator ani muscles which are already separated, but not to a maximum extent. He also admits by his experience that he cannot entirely control laceration, but more laceration occurs after he has already made the incision. It seems to me, therefore, he is not going to prevent very much by his procedure. He is performing an operation which will have no effect in preventing a troublesome form of prolapse which we know occurs, and he can only partially prevent the form of pro-

lapse which may result from laceration of the cervix. The latter, unless deep and extending into a part of the rectum, is not followed by prolapse at all, and he is not going to prevent anything like a hundred per cent of cases.

It seems to me, these are serious objections to the operation. I am in favor of making normal labor as simple a process as possible. We have to remember that the number of women in hospitals is small; the majority of women are confined in their own homes under the care of general practitioners, and the technic of Dr. DeLee is a hospital "stunt," and not one for the general practitioner. It is not right, moreover, that hospital practice should entirely outrun contact with the general practitioner. His students are coming to the hospital, they see the operation, and it is all very well to tell them not to do it, but they will do it because they have seen it done, and the results will be very different from what they are in his hospital. We ought to make the conduct of normal labor as simple a matter as possible, because it will be from the standpoint of the country at large in the hands of comparatively unskilled men, and the harm which may be done by meddlesome midwifery, although his operation is designed to correct it, may in a few years become widespread. I have no objection to hospitals being kept to the front, but they ought not to get entirely out of touch with general practice. What is the matter, as a preventive, with properly sewing up the ordinary perineal laceration which is so frequently found? If we taught students how to sew up these lacerations properly by vaginal stitching and taught them aseptic methods, in my opinion we would do more to prevent prolapse than by Dr. De Lee's operation.

Dr. John O. Polak, Brooklyn, New York.—I think the point of Dr. DeLee's paper is to distinguish between what is safe for a specialist to do and what is safe to teach students to do. I am in agreement with a great many of the points Dr. DeLee has brought out, particularly his use of an anodyne for the dilatation of the cervix, and this brings out the point that I have inspected a very large number of cervices after pituitrin has been used when the entire cervix has been effaced, and the diameter has been at least seven centimeters, and have seen these cervices torn with a fresh tear extending up into the fornices.

In regard to the delivery, he does save something. He saves more children by auscultation of the fetal heart when the head is on the pelvic floor than by discission of the pelvic floor. We have saved more children since we have adopted this routine in the last six years.

I do not agree with his lateral discission. I do not believe from the results I have had with it that it compares with the suggestion Dr. Pomeroy brought forward before this Society of median discission, and these are the reasons: Union of a belly of a muscle cannot be obtained as can fascial union. Allowing the head to remain on the pelvic floor for a long time dilates, as the doctor has said, the median fascia. Furthermore, it does something else, it dilates the anterior fascia. I have seen women who have had spontaneous labor come back with cystocele as a result of the prolonged second stage of labor. As Dr. Pomeroy has suggested, we now make a median discission after stretching the sphincter in these cases, and we have as a result several hundred women with perineums and anterior pelvic fascia absolutely intact, such as we never had before. Since we have watched other men and have seen their results, I am convinced that episiotomy is a prophylactic procedure both in the interest of the child and the woman's pelvis, whether you do it to the side, as Dr. DeLee does, or in the median line.

I am not at all in accord with his teaching about the third stage of labor. While with the woman under an anesthetic I agree with him that you can push the placenta out, as a rule, it is far better to my mind, and you have less trouble, as was shown in the work we did some years ago, by letting the placenta alone. We got a minimum amount of hemorrhage by leaving these placentas absolutely to themselves. Dr. Williams reports that the quantity of hemorrhage is in excess of what we have had. We have averaged a little less than 250 c.c. in a hundred cases.

Dr. Henry T. Byford, Chicago.—I think the whole gist of the subject is that of Dr. DeLee recommending this procedure in all cases. There is no doubt but that this should be done in cases in which the fascia begins to separate and injury is going to occur. The fact

that so many cases get well of themselves where they are left to Nature shows that the procedure should be used in the individual case, not as a routine method.

DR. EDWARD P. DAVIS, PHILADELPHIA.—We should teach third and fourth year students how to deliver normal cases without interference, and tell them to wait twenty minutes or half an hour for the placenta to come away. If a student has an abnormal case of labor to deal with that is complicated, it is as grave a case as an ovarian tumor, and he should summon expert opinion. Then that patient should go to a hospital for forceps application, embryotomy, or other procedure. I cannot see that the procedure advocated by Dr. DeLee is applicable. The fasciæ do not become obstructed necessarily until labor is fully established. When natural labor begins, if anything is done, let it be done thoroughly and surgically under anesthesia.

DR. DELEE (closing the discussion).—I regret very much I did not have five minutes more to complete my paper because a number of points subsequently raised would have been clarified.

First, as regards the danger of this operation and the matter of interference in natural labor. I consider the operation I have described is much less dangerous than the use of pituitrin to induce labor and the use of bags to induce labor at term as has been recommended to you today. I consider these procedures much more radical interferences than the method I described. In the second place, one danger only exists, i. e., doctors who have no business to do the operation are going to do it. That is unfortunate and unavoidable, and I will say no more about this phase of the subject.

Women clamor for relief from the dangers and disabilities of childbirth and we have to afford them relief. They are tired out; they are neurasthenic; they have backache, rectal and bladder trouble, and they say they never have been well since the baby was born, and we have to do something to prevent these bad sequelæ of labor. They even beg for Cesarean section to escape the dangers and pain of childbirth. They will tell you that Mrs. So-and-So who had a Cesarean section done, has no backache, while they have.

Regarding what Dr. Williams said about introducing the hand into the uterus; it is unnecessary to put the hand high into the uterus by this method. The placenta pouts down into the vagina under the influence of pituitrin and all you have to do is to press on the fundus and it falls into the basin. Where it does not come out of the vagina you put your hand into the cervix, and the placenta will slide down like the heel along a shoehorn. I am not afraid to put my hand into the uterus.

Regarding the remarks of Dr. Eden, if he had heard the rest of my paper he would have learned what incision of the pelvic floor and perineum prevents. It absolutely prevents cystocele. There is extreme circular tension of the fascia at the pelvic outlet; an incision in the posterior quadrant of the fascia relieves pressure on the pubouterine ligaments, the subvesical or vaginal fascia (which is so important in the operation for cystocele), and if the operation of prophylactic forceps does nothing else than to prevent cystocele, I would present it to you with supreme confidence.

Dr. Eden stated that the levator and pillars are already separated when I do the operation. That is not so. The right time to perform the operation is when the levator ani just begins to feel the crushing effect of the head.

About sewing up lacerations that occur in natural delivery, I wish to say that I have sewed up thousands of lacerations after delivery and have never been fully satisfied, and I claim no lack of skill; but when I sew up a perineotomy wound I can distinguish fascia and muscles beautifully. When I get a laceration to sew up where the head has been pounding and grinding the muscle like a piece of steak is pounded with a mallet, I cannot get good results.

In what other respect is the procedure prophylactic? Where the baby's head is crowded through a contracted brim you know what has happened to the brain and its vessels. There are minute and larger hemorrhages. The same is true when a head is driven through a tight outlet.

In going over the history of primogeniture we learn that the first born had a high mortality and morbidity and that children of subsequent labors, not the first labors, were people who moved the world. Benjamin Franklin was the seventeenth child.

I say that this procedure saves the mother from exhaustion and hemorrhage. I disagree with those who say that women may lose 500 c.c of blood with impunity. The less blood a woman loses at labor, the better.

Dr. Byford said something about the number of cases that go through labor spontaneously and have good perineums. I have yet to see an anatomically perfect perineum in a woman that has had a spontaneous delivery of a normally large child at term. I have had in the last two years over 200 private cases, and of this number 85 were forceps applications, and 39 cases of prophylactic forceps. I do not do the operation in every case. Most of the cases of multiparæ with large pelves do not need prophylactic forceps.

In the remarks made by Dr. Davis he implied that what we teach our students is different from what we practice, and I agree that we should teach this method only to our advanced scholars.

Dr. J. Whitridge Williams, of Baltimore, read a paper on **The Value of the Wassermann Reaction in Obstetrics based upon the Study of 4547 Consecutive Observations.**

Dr. Williams said that in 1915 he studied the fetal and infantile deaths in a series of 10,000 consecutive deliveries, and in his presidential address before the American Association for the Prevention of Infant Mortality, stated that syphilis was the most important single cause and constituted the etiologic factor in 26 per cent of the deaths occurring in his service between the end of the seventh month of pregnancy and the two weeks immediately following delivery.

As the result of that study, he concluded that the most immediately fruitful field for prenatal work lay in the earliest possible recognition of the existence of syphilis and its intensive treatment during pregnancy and that this could best be accomplished by making a Wassermann test upon every patient entering the service. Consequently, from April, 1916, to the present time a specimen of blood is withdrawn from every patient at her first visit to the dispensary, and if a positive Wassermann is obtained, she is subjected to intensive treatment in the hospital department of syphilis. Furthermore, in the hope of increasing our knowledge concerning the incidence of the disease, as well as its clinical significance for the mother and child, a Wassermann is likewise made from a sample of fetal blood obtained from the umbilical cord immediately after delivery. Every placenta is weighed and described macroscopically, after which portions of it are hardened, cut, stained and subjected to microscopic examination. Finally, whenever the child is born dead or dies within the first two weeks of the puerperium, every effort is made to secure an autopsy, at which particular attention is paid to the detection of syphilitic lesions, and a positive diagnosis is not made unless spirochetes can be demonstrated in the fetal organs.

The present work is based upon the study along these lines of 4000 women and their children who were delivered in the Johns Hopkins service, out of 4547 admissions, between April, 1916, and December 31, 1919. The patients were almost equally divided between whites and blacks, though the latter slightly predominated—1839 and 2161, respectively.

On this occasion he desired to refer incidentally to the part played by syphilis in the causation of fetal death, then to consider more fully the significance of the maternal and fetal Wassermann, next to take up briefly the value of the microscopic examination of the placenta in the detection of syphilis, and finally to say a few words concerning the status of Colles' law. The subject of syphilis as a cause of fetal death was considered in detail in an article on "The Significance of Syphilis in Prenatal Care and in the Causation of Fetal Death," which appeared in the *Bulletin of the Johns Hopkins Hospital*, May, 1920.

That article was based upon the study of 302 fetal deaths, 99 whites and 203 blacks, which occurred in the 4000 deliveries under consideration. It showed that syphilis was the most important single cause of death, and that it was responsible for 34.4 per cent of the total number. These figures did not include the children discharged alive with hereditary syphilis, or those in whom the disease developed later. Furthermore, it was shown that syphilis was responsible for more than twice as many deaths as the next most important cause, namely, dystocia, as well as for nearly as many deaths as the next three most common

causes combined, namely, dystocia, toxemia, and prematurity. The figures indicated that these three causes were responsible for 37 per cent of the deaths as compared with 34.4 for syphilis. At the same time, he pointed out the great difference in the incidence of the disease in the two races; syphilis being responsible for 12 out of the 99 white, as contrasted with 92 of the 203 black deaths, an incidence of 1 to 8, and 1 to 2, respectively.

After these preliminary remarks, Dr. Williams took up the study of the significance of the Wassermann reaction in pregnant women. In the 4000 women delivered during the period under consideration, 449, or 11.2 per cent, presented a positive reaction during pregnancy. Its incidence was much greater in the black than in the white women, being 16.29 per cent and 2.48 per cent, respectively. In other words a positive Wassermann was noted in every sixth colored woman, as compared with every fortieth white woman.

What this means serologically, it is impossible to state at this time, but practically it means that in this material, about one baby in one hundred (43 out of 4000) will have syphilis even if the maternal Wassermann is negative. Consequently one is not justified in claiming that the most ideal prenatal care can entirely eradicate the disease as a cause of fetal death. Of course, in practice the results will not be quite so bad as here indicated, for the reason that the condition would probably be recognized after the birth of the first syphilitic child, when the mother would be properly treated with a reasonable prospect that future children would be exempt from the disease.

The practical bearing of this aspect of the problem may perhaps be elucidated by a little calculation. For example, if it is assumed that 11 per cent of our women have a positive Wassermann and that, without treatment one-half of their children would be syphilitic, we should expect 55 syphilitic children in every 1000, plus 10 others (1 per cent), which would be born of women with negative Wassermann, or a total of 65 per 1000. Consequently, our figures indicate that even though routine Wassermann tests were made early upon all pregnant women and efficient treatment instituted at once, only five sixths ideal results would be obtained. Of course, this would apply only to the first delivery in the service, as in women with negative Wassermanns the existence of syphilis would be detected after the birth of the first child, when treatment would be immediately instituted afterwards and be followed by excellent results in the future.

Generally speaking, Dr. Williams felt justified in concluding that such a result should not discourage us, for if we were able to reduce the fetal mortality from syphilis by five sixths, its eventual incidence would scarcely exceed 1 per cent, and it would be converted from the most common cause of fetal death into an infrequent one.

Turning to the consideration of the significance of the fetal Wassermann at the time of delivery, the material shows that a positive result was obtained in 36 of the 4000 observations, approximately 1 per cent. This means that only a small fraction of the children born of mothers with a positive Wassermann present such a reaction. It should, however, be remembered that macerated children are not available for the tests, as their blood is already "laked."

In order to arrive at a conclusion as to the value of such investigations, an attempt was made to determine the fate of the 36 children concerned. For this purpose, they were visited at their homes, were subjected to a careful physical examination, and a sample of blood was removed in order that the Wassermann might be repeated. As three years had elapsed since the oldest cases had been discharged, it is not surprising that nine of them could not be located, thus leaving 29 available for consideration. Fourteen of the children died within the first month, mostly in the service, and in 12 of them syphilis was demonstrated. Five of the children developed clinical syphilis later, while 10 others presented no clinical signs of the disease. In these a Wassermann repeated months or years after the original gave positive results in 5 children, while it was negative in the other five.

In other words, out of the 29 children which presented a positive Wassermann at birth, 17 developed definite evidence of syphilis; 5 showed no clinical signs, but continued to have a positive Wassermann, while 7 showed no signs in association with a negative Wassermann. Accepting the positive Wassermann as conclusive evidence of the existence of syphilis in the child, it appears that the primary reaction corresponded with the clinical and anatomical findings in 76 per cent of the cases.

During the course of years, Dr. Williams became convinced from the routine microscopic study of the placenta that the syphilitic lesions occurring in it are extremely characteristic, and afford more conclusive evidence of the existence of syphilis than the demonstration of a positive maternal Wassermann, and in general tally fairly closely with the autopsy findings in the child. For this reason he was curious to ascertain in how far the results of the present study would sustain such conclusions.

From these calculations, it appears permissible to assume that the present investigation indicates that the microscopic examination of the placenta tallies with the clinical and anatomic findings in the child in from 80 to 90 per cent of the cases, which is in marked contrast to the 40 per cent obtained from a positive maternal Wassermann. Consequently, it was pleasing to find that this study confirms previous impressions, and indicates that the demonstration of the so-called Frankel's disease in the placenta offers twice as great a probability of giving correct information concerning the condition of the child as a positive Wassermann on the part of the mother and, in the absence of a carefully conducted autopsy, constitutes the most reliable means of diagnosis at our disposal.

DISCUSSION

DR. GEORGE GELLHORN, ST. LOUIS, MISSOURI.—The investigations of Dr. Williams will serve to settle definitely several mooted questions connected with the problem of syphilis in pregnancy. My own statistics obtained from a much smaller material reflect in all essentials the imposing figures of Dr. Williams. I will, therefore, not take the time to submit them in detail at this moment. I merely wish to inquire in regard to two points made by Dr. Williams. The first concerns the occasional birth of a syphilitic child by an apparently healthy (Wassermann negative) mother. As you have just heard, Dr. Williams inclines to the possibility of a gradual immunization of the mother by the continuous casting off of antibodies or chorionic villi into the maternal circulation. But I still prefer to consider Colles' law as incompatible with our conceptions of the pathology of syphilis. I prefer to explain it by such eccentricities of the Wassermann reaction with which Maud Menten made us acquainted about two years ago. As you remember Menten found in a considerable percentage of her cases that a strongly positive Wassermann would turn into a negative one immediately upon delivery, and this happened irrespective of whether or not the patient had received treatment.

The second point is of even greater practical importance. The excellent results of Dr. Williams with energetic treatment during pregnancy should forever quiet the fears of those who object to such treatment because of danger to the child. It may be that some children are born dead in spite of the treatment, but such instances do not mean a real loss to society. On the contrary, they relieve the parents and the community of a serious burden. But there is a much wider outlook to the whole question. We know that our homes for the feeble-minded, our asylums for the blind and deaf, our houses of correction, and our jails are peopled with congenital syphilitics. Is it possible to reclaim at least a number of these unfortunate ones for society? A Swedish physician, Dr. Welander, a man of deep humanity and wide vision, was to my knowledge the first to attack this problem. He founded, in 1900, a home for hereditary syphilitics, and found that if energetic treatment was instituted early enough and continued for four or five years, a very large proportion of his little patients measured up to healthy children, physically, intellectually and morally. Welander's example was emulated in Germany both by private and municipal initiative and here, too, favorable results were observed.

At the Children's Hospital in St. Louis similar efforts have been made for the last four or five years and Dr. Jeans, who conducts the work, expressed himself very optimistically to me. The one thing all these pediatricians deplore, is the fact that they do not get the children early enough, but if Dr. Williams' plan be systematically carried out by all obstetricians, all babies would get their first treatment *in utero* and before the disease has had time to make headway. They would thus get a proper start in life and the obstetrician by anticipating the work of the pediatrician, would benefit not only the individual but the entire race. Let us, then, support by all means the appeal of Dr. Williams for the efficient treatment of syphilis during pregnancy.

DR. REUBEN PETERSON, ANN ARBOR, MICHIGAN.—When I received notice that Dr. Williams was to read a paper on this subject, I tried to get some statistics from our clinic, inasmuch as we have been interested for a number of years in this question. Unfortunately I was unable to obtain all the data I desired. However, the data I have collected I thought might be of interest, as they include the Wassermanns from a large number of patients admitted to the hospital, besides the patients in the obstetric and gynecologic clinic.

It would seem at first that it might be difficult to obtain a Wassermann from every patient admitted to a large hospital. We have found no difficulty whatever at the University Hospital. When patients apply for admission a Wassermann is taken in every instance and this procedure shows some rather interesting findings. The examinations include patients from all departments. There are a number of patients distinctly syphilitic applying to the department of dermatology, but still there are large numbers in the general hospital who show positive Wassermanns. Out of 5965 patients admitted in six months, there were positive Wassermanns in 19.6 per cent. That is a surprisingly large percentage according to my reading.

Turning to the Department of Obstetrics and Gynecology, there were 1348, a comparatively small number as compared with Dr. Williams clinic. It is interesting, however, to check up on Dr. Williams' statistics, as only occasionally do we have a colored patient. Our statistics show that there were 643 Wassermanns taken of the mothers, with 44 positives, or 6.4 per cent.

Turning to the children (in the obstetric department of the clinic) we find that of 671 children examined, practically the same number were positive, that is, 6.7 per cent. If I am correct, this is a much larger percentage than Dr. Williams obtained. But in the women applying to the gynecologic clinic, we find that in 1247 examinations there were 114 positive Wassermanns. In other words, the women who applied for treatment, for diagnosis, and so on, in this clinic showed a 2 per cent higher rate than in the obstetric clinic, which was surprising to us, for we thought the ratio in the obstetric clinic would be larger than that in the gynecologic clinic because a large proportion of the children born of these women are illegitimate.

I also have had a record made of every placenta examined microscopically, and I am quite sure without investigating these cases the results will bear out what Dr. Williams has stated, namely, that the Wassermann is only a check. It is a stimulus to investigation, but the real knowledge of whether a woman is syphilitic will come better from an examination of the placenta than from the Wassermann because there are so many factors in the Wassermann examination that contribute to make it doubtful.

DR. JOHN O. POLAK, BROOKLYN, NEW YORK.—In our prenatal clinic, at the Long Island College Hospital, largely through the work of my associate Dr. Beck, we have been making Wassermanns in every patient who applies. It is interesting to note that a history of syphilis has had little value in our investigations save in the cases where there has been a history of repeated miscarriage and, unless a routine Wassermann is made on every patient who applies to the prenatal clinic, syphilitic cases may escape notice. We tried for a number of years to make Wassermanns on suspected cases and have been surprised to see how few reactions we obtained, but since we have been making a routine Wassermann of every prenatal case, our statistics are similar to Dr. Peterson's. We are dealing with white women; we have very few colored women, and we have about 6 per cent of positive Wassermanns. Where a positive Wassermann results, the test is repeated as we have found there is a discrepancy in the Wassermanns unless they are repeated, always twice, sometimes oftener. We have only checked up the positive Wassermanns that have come back after two positive examinations.

Another point of considerable interest is this: Immediately on the discovery of a positive Wassermann in a pregnant woman, no matter at what stage of pregnancy she is, we should give her salvarsan, even if it is a week before labor. It has been brought to our attention that women who have had positive Wassermanns and who have had an injection of salvarsan prior to their delivery give birth to a child which is more likely to survive, for the baby has received prenatal treatment and is not so likely to succumb to the pneumonias and other conditions prevalent in the first week or so after delivery.

One other interesting fact is that during the last year wherever women had a positive Wassermann and received salvarsan, we have had only one stillborn child due to syphilis.

DR. HAROLD C. BAILEY, NEW YORK CITY.—This subject is so important from the standpoint of the reduction of stillbirths that, it seems to me, we should have a little more data regarding the problem of the child and what will be the future of these children we have saved, and how far shall our advice go with reference to further treatment. We have had three instances in the past month in the Bellevue Hospital service, in two of which the diagnosis of spirochetæ was made by the microscope with dark field, with apparently normal children, and one woman with a positive Wassermann with a normal child. The question is, how far shall our treatment extend with negative Wassermanns in these babies? There will come a time when these Wassermanns will become positive in the infants and they should be treated as syphilitic infants. Shall we inject salvarsan into the longitudinal sinus of the baby, or adopt the mercury inunction method, which is seldom followed out?

DR. WILLIAMS (closing the discussion).—It is very interesting to hear that Dr. Peterson observed a much higher incidence of positive Wassermann reactions in his white women in Michigan than we have in Baltimore. I cannot account for it, except that in a general way it confirms the experience of his colleague in pathology (Professor Warthin) who found if I remember correctly that 40 per cent of all the autopsies which he performed in Michigan showed signs of syphilis. If he is correct, it would seem that Dr. Peterson's figures are surprisingly small.

In regard to what Dr. Polak has said, I can only endorse his statement that in a large number of cases we were unable to elicit a definite history of infection, as in our entire series we could obtain a history of a primary sore in only a small number of cases, while a history of secondary or tertiary lesions was lacking in 80 per cent of the patients. In other words, it is my experience that if we were to rely upon the history alone, four-fifths of the patients would escape treatment.

Regarding repeated Wassermanns, I can only say that they are repeatedly made upon our patients, for, whenever they present a positive Wassermann, they are not treated by us, but by the department of syphilis of the hospital. In that service a Wassermann is taken with each dose of salvarsan, so that in the patients who are efficiently treated, we have a large number of controls. After the patient has had six salvarsan injections she is given a course of mercurial treatment, and if at its conclusion the Wassermann is still positive, another course of salvarsan is given.

In general, I think that the most important fact derived from our investigations is the very wonderful results which we obtain from efficient prenatal care, and my paper shows that one of the important duties of the prenatal clinic is to detect the existence of syphilis at the very earliest possible period of pregnancy and to treat it energetically. In my experience the difficulty exists not in making the diagnosis, but in persuading the women to return at regular intervals for treatment. As we have developed this work, it has been necessary to increase greatly the number of prenatal workers, so as to educate the women to return regularly. After delivery the cases are followed up to ascertain what happens to the mother and the child, and the amount of work which is required to get ideal results in this respect is appalling.

DR. JOHN A. MCGLINN, of Philadelphia, read a paper entitled **Extraperitoneal Cesarean Section.** (For original article see page 45.)

DISCUSSION

DR. JOHN O. POLAK, BROOKLYN, NEW YORK.—After the arraignment Dr. McGlinn has made of extraperitoneal procedures, I have some hesitancy in opening this discussion.

I have had no experience with the true extraperitoneal operation. The last one performed in our clinic was done by the late Dr. Skene and the last one I saw performed was about a year ago at the New York Lying-In Hospital. When I saw Dr. Skene do the operation it impressed me as a bloody procedure, done out of sight, and had none of the advantages that a surgical procedure should have.

We have done a very large number of classical Cesarean sections, and we feel that there are some objections to it. We have reopened 17 of these classical cases. We studied the scars in these 17 cases; we found adhesions of the intestines to the scar in 5; we found weakened scars in 12 covered by omental attachments. For this reason and on account of the morbidity we were getting, we analyzed our cases and at the suggestion of Dr. Beck we have done a modification of the Krönig operation. We believe that infection does not come from the spill; we believe any surgeon can protect the abdomen from the spill in the classical operation.

We furthermore believe that infection comes from the endometrium and in our autopsy observations after the classical operations, where the patients have died of peritonitis, we found that along the suture line we had the same picture that presents itself when we have suppuration of the abdominal wound around silkworm gut suture. The infection came from the endometrium, which is admitted to be the most frequent site of infection of the uterus. It is the carrying in of the flora from the vagina into the uterus that gives rise to the endometritis and the suture carries it through to the peritoneum. After doing a number of Krönig operations, I tried the Veit-Fromme-Hirst operation, but found we did not protect the peritoneum so we are now doing Beck's modification of the Krönig. We have the scar low down under the bladder reflection and, as a result these patients do not have adhesions and convalescence is smoother.

Another point of great interest is that we have done this operation in the frankly infected cases. We have no instance of dystocia in cases where we have resorted to this procedure. In an experience of 260 classical operations we have only had to do 2 Porro operations. I do not believe the universal principle that has been taught that in each case of infection a Porro operation is the only thing to do, is confirmed by our experience. The principal point to my mind of the transperitoneal operation is that those patients who are suspected of being infected can have their uteri saved.

In regard to rupture, all of us have done anterior vaginal hysterotomy; all of us have effected subsequent deliveries through the scar. What is the difference between a low operation and vaginal procedure? Rupture through the scar in this location is not a serious matter as the rupture is under the bladder reflection. We have seen them dilate; some of them rupture, but we have never seen a fatality in these cases and we have done a large number.

DR. JOSEPH B. DELEE, CHICAGO.—I am much interested in these newer forms of Cesarean section. I have given up true extraperitoneal section, that is, where the peritoneum is preserved and no opening made into the peritoneal cavity. I have noticed, too, in reading the later European literature that over there the extraperitonal Cesarean operation is being displaced by the low cervical Cesarean, and the high or corporeal, or old classic, Saenger, or as it should be called the Saenger-corporeal or fundal-Cesarean section, is discarded for the low cervical. I claim the low cervical Cesarean section has distinct advantages over the classical and I perform it in nearly every case except for certain conditions. These conditions are getting fewer.

I would like to ask Dr. McGlinn if in the course of his study of the literature he has found a single case of rupture of the uterus after cervical Cesarean section? Considering the thousands of operations that have been done already, we ought to have some cases of

rupture of the uterus. I have come across three cases of rupture of the uterus. In two reported by Küstner, the rupture was in a portion of the incision which had extended into the fundus. The other of the three was in a woman who had had a Cesarean section, went into labor and burst the uterus, not at the site of the incision, but to one side. I would be glad to get a report of some authentic cases of rupture of the uterus.

I have done 50 cervical Cesarean sections where the cut lies in the same location, and I have not yet had a case return to me or a report of rupture of the cervical scar.

Dr. McGlinn made the statement that the operation cannot be done in placenta previa. I used to think so too. I discovered a placenta previa in a case the other day in which I resorted to cervical section. There was less hemorrhage in this case than in some cases where there is no placenta previa.

Lichtenstein in 1919 reported on low cervical Cesarean section, and in 4 of his cases the placenta was attached to the anterior wall. All recovered. We have had 50 Cesarean sections of the low cervical variety and no deaths except in one case, and I feel I should have done a craniotomy in that instance. The woman was in labor four days; she had a blood pressure of 208 mm., with albumin and casts in the urine. Her urine was smoky. She had intertrigo all over the abdomen. The head could not engage. She weighed 278 pounds. As I have said, I should have done a craniotomy in this case. This woman died of peritonitis. I expected too much from the operation. All the other cases made fine recoveries. Two had suppuration.

Dr. McGlinn said that this operation will never generally be adopted. This is probably true of the extraperitoneal method. Our German confreres are performing this operation in cases that are frankly infected. In 500 cases reported by 4 operators, the mortality was 2½ per cent. The absence of adhesions in these cases is remarkable. It has been stated that the operation cannot be done on the same patient twice. That is not true. I have done it on two patients twice. Litchenstein has done it three times on a number of patients. In my two cases the peritoneum hardly showed the scar.

I wish to suggest a little improvement in the technic. If you make a careful dissection of the peritoneum downward off the anterior wall of the uterus where it reflects on the bladder, if before you cut into the uterus you move the tissues a little, you will see a distinct layer of fascia. You can slit the fascia down individually or you can make a cut into the uterus, cutting the fascia at the same time as you do the muscle of the cervix. When you come to sew it up you do it in the same anatomic manner. In the last three or four cases I made a separate line of sutures in the fascia, so that there were two rows of sutures in the cervix, one row in the fascia, and a flap of peritoneum, which is a great protection against leakage from the uterus, and gives the firmest possible uterine scar.

Dr. Edward P. Davis, Philadelphia.—One of the worst cases I have ever seen was a Cesarean section done by myself on a woman upon whom previously this kind of extraperitoneal operation had been done by a skillful surgeon. The adhesions were the most extensive I have ever seen in any abdomen. I would therefore question the statement made that adhesions cannot form after the operation. Extraperitoneal Cesarean section is evidently a practical impossibility, but it is a curious thing that a few months ago the old Thomas elytrotomy was actually in process of revival and was being done in this country, so that there is a great tendency to seek some method of extraperitoneal operation. It should only be done in very difficult cases, but even then it cannot be considered dependable.

Much has been said concerning infection. How can you deliver a woman who has been long in labor and dangerously and possibly fatally infected? If you attempt to make bacterial culture, must you judge by the appearance of the decidua or the odor? I can only say that in a considerable number of cases of suspected women in doing the classic section the uterus has been drained by packing carried through the vagina. Ergot and strychnine have been given hypodermically to induce firm uterine contractions. These women have had sapremia, have lived, and maintained the function of reproduction. While that is a gamble, are not many surgical operations in some sense a gamble? In the frankly infected cases I think the majority of us are agreed that safety lies in one direction, if infection can be admitted to be present, namely, we should resort to section with the amputation of the uterus and leaving the stump outside of the peritoneal cavity.

I should be glad to see any practical demonstration of the value of the transperitoneal section, but I believe extraperitoneal section has proved a practical impossibility.

DR. RUDOLPH W. HOLMES, CHICAGO.—My whole experience has been negative in the employment of the newer methods of Cesarean section. I may be erroneously satisfied with my own work, but I feel that the dangers do not rise or fall by any particular method or technic of operation, but are directly due to the conditions present before, during, and after operation. I have always eventrated the uterus, packing off the bowel, and closing the abdominal wound behind the uterus by means of a bullet forceps, have always torn open the uterus after a preliminary outline by means of a scalpel. I have now a considerable series without any mortality. Essentially, the plan I follow was instituted about fifteen years ago; in the previous years I had three fatalities in the first ten sections. I have never performed a section on the pelvic indication before labor, except it be a repeater or there was an absolute indication, unless there had been a thorough test of labor. My experience convinces me that the mere length, prolongation, of labor places no jeopardy upon the mother or baby, provided her vital forces have not been sapped, as shown by a rapid pulse, elevation of temperature, or the facies of exhaustion, nor upon the child provided the heart tones have not radically altered in character. A truly tired woman is not a safe risk for any operation. I have done Cesareans when the membranes have been ruptured many hours, even in one in which they ruptured nearly three days before without any unhappy reaction during the postoperative period. In fact, I am convinced that a true test of labor is not given the woman unless the membranes have ruptured. It should be an obstetric aphorism that an obstetrician should determine the possibility of the necessity for a Cesarean section as a mental reservation the last days of pregnancy. If this is done, she being held as a specially preferred risk, if no vaginal examinations are made, and if control of labor is obtained by rectal touch and abdominal palpation (and the stethoscope), matters may continue into the second stage, after the membranes have ruptured, without jeopardy to the mother or child. Further, it is surprising how frequently it will follow that such women will have spontaneous labors, or at most a forceps operation of expediency. Labor in women with minor pelvic contraction, with some evident slight pelvo-fetal disproportion, so conducted will only exhibit a small minority who require Cesarean.

The whole sum and substance of the problem is not to stir up the bacterial content of the vagina by vaginal examinations. If infection be suspected, the peritoneum can better dispose of the potential infection than the cellular tissue. Opening up cellular spaces by the newer methods, I believe, endangers the woman more than it prevents trouble, and needlessly complicates the technic. The next two or three years will prove whether these newer, or better rejuvenation of old methods, are not signs of our progress in the fads and fancies of obstetric practice. All the available statistics prove that Cesarean sections are followed by vulnerable scars in the proportion of 16 per cent: the menace of their rupture is a serious problem for him who attempts to conduct a later labor by spontaneous effort on the part of the mother.

The second serious problem is the inevitable liability or probability of adhesions which may cause subsequent distress. Fortunately, in all my sections, so far as I know, I had no such complications, though in the repeated sections, adhesions were found in all with one exception.

DR. MCGLINN (closing the discussion).—I am rather free to confess that had I looked up the literature on extraperitoneal Cesarean section before writing my paper, I would not have written it. However, after listening to private conversations and discussions in reference to extraperitoneal Cesarean section, and such absurd claims having been made for this operation, I was led to write the paper.

I have great sympathy for Dr. DeLee because he did not have time to finish the reading of his paper and consequently the points which he intended to bring out he was unable to do, on account of lack of time. I have lost my sympathy for him now because while I hurried through my paper, I am afraid he did not listen attentively to what I was reading. I tried to make a specific differentiation in my paper between extraperitoneal Cesarean section and the transperitoneal Cesarean section, and I still say that in civil surgery of acci-

dents extraperitoneal Cesarean section, with its difficulties, will never be accepted. Transperitoneal Cesarean section, or the modification as proposed by Dr. Beck, I do believe will meet with general acceptance because incision through the body of the uterus certainly has disadvantages that incision through the lower uterine segment does not have.

I am not aware of any cases of rupture of the uterus below the uterine scar. There have been a number of references made to such a thing, but the literature has not been available to us at the present time or before the writing of my paper.

Dr. DeLee cited the rarity of rupture after vaginal Cesarean section. In the vaginal Cesarean sections which I have seen performed, if you incise the uterus in exactly the same place where you incise after transperitoneal Cesarean section, then I have never seen a properly performed vaginal Cesarean section, or a proper transperitoneal Cesarean section, because in the vaginal Cesarean section you go through the cervix, deflect the bladder up but do not open up the peritoneal cavity, whereas in the transperitoneal Cesarean section you make the incision close to the scar which has been referred to in the literature as quite likely to occur. Undoubtedly there are some cases of rupture of the uterus although I have not been able to find them.

So far as placenta previa is concerned, the operation has been repeated several times in cases of placenta previa.

In reference to the impossibility of performing the operation, I did not say it should not be performed at all, but it should be done under positive indications. In the majority of cases it cannot be performed and that holds true with reference to extraperitoneal Cesarean section, and not to transperitoneal Cesarean section at all.

I believe so far as the value of the transperitoneal section in the frankly infected cases is concerned, the only protection we have is in the proper peritonealization of the wound. You can do the Beck operation and avoid the difficulties of adhesions and intestinal complications. It fulfills the place of the transperitoneal Cesarean section because any surgeon can wall off the peritoneal cavity as effectively as with transperitoneal Cesarean section. The operation meets every indication the same as the transperitoneal Cesarean section, it is easier to perform, and one that is absolutely shorn of difficulties. The Beck operation has a place in obstetrics at the present time. It is a great advance over the old Saenger operation with the incision made in the body of the uterus.

Dr. Hugo Ehrenfest, of St. Louis, Mo., presented a paper entitled **Recent Progress in Obstetrics and Gynecology.**

This contribution will be made the basis of collective reviews, the first of which appears in this number, page 103.

Dr. Thomas J. Watkins, of Chicago, Ill., read a paper entitled **The Treatment of Suppurating Wounds Following Abdominal Section.**

After commenting on the frequency of infected wounds when it is necessary to operate in acute abdominal conditions, Dr. Watkins called attention to the danger of contamination in cases of cancer of the cervix, especially when advanced and also in hysterectomies when a fibroid polypus protrudes into the vagina. The statistics on operation for cancer of the cervix emphasizes the great importance of thorough cleansing of the vaginal canal and cauterization of the cervix before making the abdominal section. Where a submucous infected polypus is present, it is important to remove the same at least two weeks before doing a hysterectomy. Where suppuration is anticipated, it has been customary to establish vaginal drainage with the hope of protecting the abdominal contents from infection, but Dr. Watkins stated that his experience with this procedure had been disappointing.

He believes that the general principles of treatment of infected abdominal wounds is the same as other wounds in other parts of the body and stated his conviction that the general custom is to overemphasize their treatment and consequently to delay repair, to unnecessarily disturb the patient and to impair the strength of the abdominal wall. In the treatment of

these wounds it is well to bear in mind that the infection does not long remain localized and that cases which do not suppurate are often quite as acute and prolonged as cases which develop suppuration. The presence of pus is, therefore, not of paramount importance. In many instances the treatment of the systemic infection is more important than the direct treatment of the wound and the possibility of spontaneous autovaccination and destruction of the bacteria by their own toxins is worthy of consideration. The treatment recommended by Dr. Watkins is as follows: No sutures are removed until the wound is healed unless this is made necessary by extensive cutting of the sutures into the tissues. No drainage is inserted and no probing permitted. Moist dressings of boric acid solution are kept continuously over the wound as long as it remains reddened or indurated, care being taken that maceration does not result. A large amount of drainage can effectively take place through a small opening with the use of these dressings. Antiseptics are not indicated except in wounds which contain considerable necrotic tissue, when the chlorinated solutions are of value. No exception is made in cases of infected wounds complicated by intestinal fistulæ. Drains are not employed at any time. An important feature of this treatment is that no pain is inflicted and the patient is disturbed very little.

Dr. Watkins stated that he had been using this method for about fifteen years and in his experience the results have been better than where more energetic treatment was employed. The abdominal wall has invariably been as strong as in those cases where no suppuration took place. After reporting two cases in detail Dr. Watkins presented the following summary of his procedure: 1. No sutures are removed on account of suppuration. 2. No drains inserted. 3. No probing permitted. 4. Wet boric dressings are kept continuously applied until induration and excessive redness of the wound disappear. 5. Experience shows that this treatment secures efficient drainage. 6. No appreciable cavity is present at the site of suppuration when the wound is draining; intraabdominal and atmospheric pressure keep the suppurating surfaces in relative apposition. 7. When drainage ceases no open wound exists. 8. Experience extending over fifteen years has demonstrated that with the above treatment the infected wounds have healed in less time, the patients have been much less disturbed, and the abdominal wound has been left much stronger than where energetic treatment was employed.

DISCUSSION

Dr. Thomas Watts Eden, of London, England.—I do not understand the principle on which his treatment is based but I infer that Dr. Watkins attaches a great deal of importance to the question of general systemic infection in the case of infected wounds and that therefore this treatment need not be very active. While that may be true, one's experience clinically is that when the local condition is relieved the general condition is at once relieved; that is to say, if one can relieve tension in a suppurating wound the general condition improves at once. He goes rather on the other plan and tries to relieve systemic infection before he relieves tension in his wounds. I would like to know the scientific basis for that assumption.

A general surgical principle concerning which we are all agreed and act upon is that free drainage is necessary for suppurating wounds and, although what he said about the use of antiseptics in wounds may have some justification, I think the experience of war surgery is that such wounds do not do well unless they are laid freely open, so that they can be freely drained and irrigated. We ought to apply simple surgical principles to infected abdominal wounds and it is a departure from principle to leave wounds locked up and not allow free drainage. The treatment we adopt in some cases depends a good deal upon the organism which is present. An ordinary staphylococcic infection will clear up under very simple treatment. Streptococcic infections are more serious. If there is a bacillus coli infection of the wound the proposition is even more serious. It is my practice to have a bacteriologic examination made at once of any pus that appears, and a good deal depends upon what we find. In streptococci and bacillus coli infections it is necessary to open up the wound widely. If you do that and irrigate freely, the wound will clear up, and then you can resuture it. I consider this the best treatment to adopt.

Dr. Joseph Brettauer, New York City.—I agree with Dr. Eden that we should always treat these cases according to the principles of free drainage.

What I was especially interested in and had hoped to hear explained was Dr. Watkins' statement regarding immunization.

DR. JOHN A. MCGLINN, PHILADELPHIA.—Dr. Watkins' analogy between infected abdominal wounds and acute infections of the chest, which the general surgeon has learned to let alone, is not well drawn, because if there was ever a lot of nonsense written about delayed operation it was in reference to delayed operation in cases of empyema. The advantage of delayed operation has been shown and published reports proved that some surgeons were operating at certain camps before the patients had empyema at all; in fact they were operating in cases of pneumonia, doing a simple thoracotomy or rib resection and subsequently the patients died.

Another fact to be noted is that the tremendous mortality following operation, was at the height of the influenza epidemic. When the infection began to subside and patient did not die from the primary infection, the surgeons could operate on these cases and a certain percentage would get well. As the infection became less and less, the majority of patients got well after operation, so that the analogy between the earlier drainage of the chest and delayed operations on the chest does not hold in these particular cases. Dr. Watkins allows Nature to do for him by drainage, what we have attempted to do early in the treatment of wounds, namely, to establish free drainage. In the past the use of antiseptics to sterilize a wound was not of great value, but certainly with the advent of the newer class of antiseptics that does not hold true.

As Dr. Eden says, free drainage and prompt sterilization will enable us to clean up the wounds, suture them promptly and get union by primary intention. That has been my experience in these abdominal infections, opening the wounds, establishing free drainage, and sterilizing the wounds with chlorolyptol, which is a chlorinated eucalyptus oil, or with Dakin's oil, or if there is necrotic tissue, with the hypochlorite solution, and either binding the wound up after a bacterial count is made, or simply strapping the wound carefully with adhesive plaster. In this way we are able to get as good results as Dr. Watkins showed and in a shorter space of time. I have had wounds heal absolutely in five days after opening them up and after sterilization and strapping them perfectly with adhesive plaster.

DR. CHARLES G. CHILD, JR., NEW YORK CITY.—I shall confine my remarks to two points, prophylaxis, and wound closure. It has been my experience that the material used in the wound closure has a great deal to do with infection. I do not believe absorbable suture material has any place in the closure of abdominal incisions. It is notoriously uncertain in its tensile strength and predisposes both primarily and secondarily to postoperative hernia. In the second place, absorbable material used as a suture, if it has any advantage at all it is its absorbability, which is notoriously uncertain, sometimes taking from thirty to forty days. An important point in wound closure is the fact that before such material can fulfil the vaunted virtue of absorbability, it must be converted into a soluble gelatin.

Gelatin is an excellent culture medium upon which bacteria readily grow, especially when backed up by normal tissue secretions. The tissues when in a normal or approximately normal state, will readily take care of a small amount of infection if the wound has been closed, with not infectable, and this means not absorbable, suture material, and is free from devitalized tissue. It is a different story when the wound contains traumatized tissue, in foci produced by strangulating sutures. Such a wound is an easy prey to infection. The wound should be closed with nonabsorbable material and it should be noninfective and permit of removal subsequently.

The method I prefer is continuous mattress sutures of silkworm gut or one of its substitutes, both for the peritoneum, fascia, and skin, with the ends brought out through the angles of the incision. These sutures are removed from the tenth to the fourteenth day. When I gave up the use of absorbable suture material my percentage of primary wound union immediately jumped from less than 80 to between 96 and 98 per cent, where it has remained since that time. When the wound is once infected, the relief of suture tension and free drainage are the important points.

DR. BROOKE M. ANSPACH, PHILADELPHIA.—I appreciate Dr. Watkins' point in the conservative treatment of infected wounds. There is also much to be said in favor of free drainage, but in the early stage of an infection, unless drainage is very carefully made, much harm may be done. I have more than once thought that a comparatively simple infection was made a serious one by too vigorous treatment. In the early stage it seems to me the use of heat in the form of an old-fashioned poultice, will localize the infection. After that, opening up the superficial layers and letting out pus will promote rapid recovery and healing.

There is one thing that has not been mentioned that I have used a couple of times with success, in wounds which, in spite of drainage and systemic treatment, will not heal. I have seen good results from the use of vaccines in these cases.

DR. J. WESLEY BOVÉE, WASHINGTON, D. C.—I think the prophylactic treatment of infection is important. One case Dr. Watkins presented was of cancer of the cervix. It seems to me, we can easily provide against anything but the rarest infections in this manner by not removing the diseased tissue through the abdominal opening, but by separating it from above, pushing it down through the vulva, and removing it in that way after the abdominal wound has been closed. This procedure I have followed since March, 1898, following a paper of Dr. Werder on the treatment of cancer of the uterus which appeared in the *American Journal of Obstetrics and Diseases of Women*, February, 1898. There is no danger of infection of the abdominal wound if this procedure is employed.

I take exception to the remarks of those who insist that absorbable suture material, such as iodine catgut, properly prepared, for closing abdominal wounds, is not the proper thing. I probably do not use in one-tenth of one per cent of my abdominal cases any nonabsorbable suture material in which I have had infection. In those cases in which I expected infection I have followed the plan of closing the peritoneum with a purse string suture and deluging the wound with 2 per cent permanganate solution, suturing it up in layers, closing it with buried sutures, but with no suture through the skin at all. In those in which I expected infection I have secured union, and in those in which I obtained a slight infection in recent years, I considered that they were perfectly clean.

Some drainage is an advisable thing. We should be thankful for the opening which has occurred if suppuration occurs. By changing the position of the patient, drainage is favored.

DR. EDWARD REYNOLDS, BOSTON.—It seems to me that some of the difference of opinion is from lack of discrimination. These are not war wounds. Most of our postoperative suppurations are of very mild degree. If such wounds present a purulent secretion the great majority of them will eventually get well if let alone.

Many of these suppurations originate from a bit of necrotic tissue somewhere in the wound, and I have for many years employed a preparation known as "enzymol," which is a digestive ferment. Whenever I get a necrotic wound, I put pressure on it, empty it of its own fluids, and pour in a little of the "enzymol" to fill the wound up, making approximately a 50 per cent solution. It is amazing how rapidly the necrosis will disappear and leave clean granulations. Many of them close rapidly.

DR. HENRY T. BYFORD, CHICAGO.—I have used the "enzymol" treatment exactly as Dr. Reynolds has suggested, in a 33 to 50 per cent solution. It goes in like a ferret, through the smallest openings, and destroys every bit of the albuminous material without producing any irritation. The pus should be pressed out and the wound be kept moist with the "enzymol" solution.

DR. REUBEN PETERSON, ANN ARBOR, MICHIGAN.—The treatment recommended by Dr. Watkins is rather startling at first, because it does not agree with what we have been doing with our suppurating wounds. If I understood Dr. Watkins correctly, he wants to stop the opening up of these suppurating wounds, that is, by putting on wet dressings a great many of these wounds will heal. On the other hand, we want to bear in mind that there are certain infections where this is a dangerous practice, especially when there is a large amount of suppurating material deep down. I feel safer under these conditions

to go back to our former custom of opening freely so as to get the virulent collections away from the peritoneum as quickly as possible. However, this paper is timely because it will stop opening up widely every wound that suppurates and doing a great deal of damage which can be avoided by the procedure he outlines.

Dr. Richard R. Smith, Grand Rapids, Michigan.—A good many years ago we adopted the plan as outlined by Dr. Watkins. We did this gradually, from the free opening of the wounds to doing very little or nothing. We have found it very satisfactory; we have saved patients a good deal of time in the hospital; we have saved patients the possibility of hernia, and altogether it has been a satisfactory procedure. We do not do exactly as Dr. Watkins does; we do not put on a wet dressing; we examine the wound, and when suppuration seems close to the skin we open up and encourage drainage through a small opening. We put on simple dry dressings, renew these dressings frequently, and in the vast majority of instances the wounds heal promptly without any undue expenditure of time. We make the same exception in handling these wounds of which Dr. Peterson spoke. When they do not do well we open up freely as we used to do.

Dr. Watkins (closing the discussion).—To economize time, I will attempt to reply to the discussions collectively and not individually. I am somewhat at an advantage as I have employed this conservative method and have also used the other methods which have been discussed.

After an experience extending over many years with this conservative treatment, the wound has healed in from 25 to 50 per cent less time when I used more radical treatment. Painful and disturbing treatments have been eliminated.

The principle of the general treatment of infections was not included in the paper for lack of time and is not pertinent to the discussion. The rapid general improvement, which Dr. Eden mentioned, which often follows opening an infected wound, is interesting and important. It is, however, not as important as might be inferred, because with the treatment outlined, the rapid change for better is generally as early and as complete as he has mentioned.

The literature and reports relative to the treatment of infected wounds during the war, has failed to add much, if anything, to the treatment of wounds in civil practice. The war wounds were generally contaminated and contained much devitalized tissue. Dr. Frederick A Besley, of Chicago, who had an extensive overseas service, informs me that he believes that the antiseptic treatment of wounds employed, did more harm than good, but that the use of chlorine solutions was of value when wounds contained much devitalized and contaminated tissue. The digestive fluid recommended by Drs. Reynolds and Byford would probably act much in the same way and is possibly of greater value than the chlorine solutions.

I believe the variety of infections should not determine the kind of treatment to any great extent. There would be less objection to active treatment for a staphylococcus infected than for a streptococcus infected wound, because the streptococcus infection is easily disseminated and embolic disturbances readily follow.

The discussion intimates that I do not drain infected wounds. My contention is that the treatment recommended gives efficient drainage. When the dressings are removed and the patient's position changed, the wounds are found well drained. This favorable result does not always obtain when tubes and gauze are inserted for supposed drainage purposes.

Dr. Lillian K. P. Farrar, of New York, read by invitation, a paper entitled **Acidosis in Operative Surgery and Its Treatment by Glucose and Gum Acacia Given Intravenously.**

Dr. Farrar presented an extended study of this subject based on observations made at the Woman's Hospital of New York. In reviewing the physiologic basis underlying this procedure, Dr. Farrar called attention to the fact that during any surgical operation the acid

by-products of the body are greatly increased. If the bicarbonates of the blood which carry the acid carbon dioxide gas to the alveoli of the lungs are present in large amount, the combining power of the blood, i.e., the ability of the bicarbonates to unite with the CO_2, is high and lung ventilation is maintained. On the other hand, if the bicarbonates are low or the acid by-products greatly increased, a condition of intracellular acidosis results which may endanger the life of the patient if the operation be prolonged.

Carbohydrates are necessary for the complete oxidation of acetic acid. Glucose is normally present in the blood and is assimulated in this form without further metabolism. An individual will absorb 0.8 grams of glucose for each kilogram of body weight without the production of a glycosuria and this rate can be maintained for several hours if desired. Gum acacia is a colloid of the same viscosity as that of the blood and, when added in a 6 per cent solution to a 20 per cent glucose solution, will keep in the blood stream the water which glucose attracts from the tissues. A satisfactory blood pressure is thus maintained and the loss of bicarbonates into the circulation prevented. If blood pressure is maintained and the acid by-products completely oxidized, acidosis occurring during operation will be prevented. Salt solutions do not maintain blood pressure, as the salt is taken up by the tissues as the latter attracts the water. A small volume of a higher viscosity such as glucose and gum acacia is therefore preferable because the blood is not diluted and the burden on the heart is consequently lessened.

Dr. Farrar in summarizing the indications for the procedure stated that given during operation it had been found of great value in combating acidosis and preventing postoperative vomiting, in cases of shock where it was necessary or desirable to maintain a normal blood pressure, in hemorrhage and to promote diuresis. In their service at the Woman's Hospital it was given when a patient's resistance had been shown by a preoperative examination to be lowered, where a long or severe operation was anticipated, and in shock or hemorrhage associated with ruptured ectopic or similar accidents. Another class of cases in which glucose was administered with good results after operation included severe postoperative vomiting and in peritonitis where the food value of the solution sustained the patient until able to take nourishment by mouth.

The technic of the procedure included the administration of bicarbonate of soda in doses of from one-half to one dram every three or four hours for two or three days previous to operation. The patient is given a meat-free diet, with starchy foods, fruits, vegetables and an abundance of water. This contributes to the alkali reserve. The injection is made with a salvarsan apparatus, using small needles. The temperature of the solution should be 105° F. as it enters the vein. The quantity of the solution and rate of flow is governed by the patient's weight, thus a woman weighing 100 to 125 pounds can be given 200 c.c. per hour, or about 3 c.c. per minute. A patient weighing 125 to 150 pounds can be given 250 c.c. per hour or about 4 c.c. per minute. These figures are obtained by Woodyatt's method and are applied to every patient. The postoperative diet in acidosis should consist for the first twenty-four to thirty-six hours of the juice of citrous fruits, cereals with cream and sugar (except oatmeal), toast, jelly, honey, malted milk, custard, junket, bread and butter, rice or bread pudding with cream and sugar as freely as desired. Bicarbonate of soda 30 to 60 grains every three or four hours will also assist in lessening postoperative acidosis.

The gum-glucose solution as employed by Dr. Farrar is made from the powdered Egyptian gum Arabic and preferably the anhydrous chemically pure dextrose. Great care must be exercised in its preparation, for the details of which attention is directed to the complete paper which will be published elsewhere at a later date.

DISCUSSION

Dr. Rollin T. Woodyatt, Chicago (by invitation).—There is a great tendency in using means of this sort to group certain conditions under certain clinical names, such as shock, acidosis, and so on, without entirely realizing the combination of conditions which may occur in different individuals; that is, one case of shock is not necessarily like another one; hence a clear understanding of the basic principles underlying the use of these agents is important.

Gum acacia, glucose, and salt are all similar to each other in one respect, as Dr. Farrar has pointed out, in that they all have high hydration capacities and will hold water in combination with themselves. Whether in the bowel or kidney tubules, you will find these substances associated with water and, of course, as they pass into the blood stream they accumulate water in the blood creating the condition of hydremic plethora. Thus they increase blood volume. But these substances also differ in other respects.

An inorganic salt, like sodium chloride, when introduced into the blood passes rapidly from the blood into the tissue cells, and being nonoxidizable, rapidly reverses the initial hydremic plethora and replaces it with hydrops of the tissues. Glucose differs from salt in the respect that after passing from the blood into the tissues it undergoes utilization (oxidation, storage) and does not accumulate as unchanged glucose in the tissues. Instead of exhibiting the hydrops producing action of salts in the tissues, glucose substitutes for these the beneficial chemical effects which result from its utilization and, as Dr. Farrar has stated, these include the increased storage of glycogen, an increased energy in nonstriped muscular fibers and increased tone, so that following glucose injections it is not uncommon to find a marked increase in the tone of the musculature of the alimentary tract from the stomach down, an effect resembling, though not quite so striking, as that of pituitrin. Gum acacia has a certain advantage over salt in that it does not diffuse into the tissues so rapidly. It therefore cannot accumulate in the tissues but remains in the blood and holds water in the place where it is wanted. It has a certain advantage over glucose, owing to the fact that after having been introduced into the blood stream all at one time the supply need not be maintained by continuous injection. It does not take the place of glucose in all respects because it exhibits none of the chemical effects of glucose in the tissues. Therefore, a combination of gum and glucose is a rational procedure.

I personally have not used gum acacia, but have been more concerned with a technic enabling us to sustain the effects of glucose as long as we wanted them by an apparatus which permits the continuance of injection for an indefinite time. Basing my remarks on the use of glucose without gum I would emphasize a point brought out by Dr. Farrar, to the effect that the increase in blood presure, both in its height and duration, is greater than that obtainable with sodium chloride. The improvement in the condition of the patient in shock and shock-like states, particularly when there has been considerable dehydration, is more marked following glucose injections than after the use of salt solution alone, and with glucose there is no subsequent reversal of the first effect on the blood volume. So far as it goes and while it lasts the effect of the glucose is an unmitigated good. Salt has a transient effect on the blood volume and a doubtful effect on the mean arterial pressure followed often by hydrops.

In cases of resection of the bowel, in certain cases where there has been stenosis of the pylorus, with marked dehydration or failure of the body to absorb the water it takes in by the alimentary route, also in cases of "transudate" into the stomach, persistent vomiting with dehydration fever and starvation, and in several other conditions, we have obtained results with glucose which cannot be obtained by other known means.

A point which would appear to be of particular interest in relation to the gynecological operating room is the frequency, as you undoubtedly know better than I do, with which in conditions associated with pregnancy that type of acidosis occurs in which the acid accumulation is not due simply to tissue asphyxia, as in shock, but in which it is due to that derangement of metabolism which you see in fasting and in diabetes, namely, acidosis having its origin in the fatty acids and associated with acetoacetic and betahydroxybutyric acids in the urine. This type of acidosis is specifically combated by the administration of glucose.

So as to attain the best results with intravenous injections, it is desirable to take up in each case the several items—blood pressure, blood volume, plasma bicarbonate, the water reserve of the body, the degree of starvation, etc., and then to make an intravenous prescription for the individual case, carrying the treatment toward definite ends. Where the main lack is water, use a 3 or 4 per cent glucose solution; for a shock-like state without dehydration, use a hypertonic solution. If there is a very low bicarbonate reserve, add enough bicarbonate to restore it to normal and make the solution and the rate of its injection fit the case.

DR. FREDERICK J. TAUSSIG, ST. LOUIS, MISSOURI.—I should like to report briefly concerning two of my cases of postoperative shock treated by Dr. Erlanger with gum-acacia-glucose solution and included in his report on this subject. Both of them have a certain interest and lesson. The first of these was of a large abdominal tumor, a fibroid, filling the entire abdomen, and with it, an infected ovarian cyst. It was a most difficult operation in a woman whose kidneys were in bad shape, but by advice of the internist I decided to do this rather hazardous operation. Previously I had consulted Dr. Erlanger, feeling it was a case suitable for gum-acacia-glucose injections. The blood pressure within twenty minutes after the operation was begun, fell to below 100, the pulse became rapid and imperceptible, and Dr. Erlanger proceeded to inject the solution. As you may know, he uses a stronger solution of gum acacia, 20 to 25 per cent, and a little bit higher glucose also. Within twenty-five minutes the pulse pressure which had fallen to three and one-half millimeters and the blood pressure ranging from 60 systolic to 50 diastolic as registered on the apparatus for that purpose, rose to 90-65 blood pressure, and pulse pressure from 6 to 7 millimeters. The patient at the time she was removed to her room was in as fair condition as one could ordinarily expect any woman to be after a laparotomy. Unfortunately this patient twenty-nine days after operation died of pneumonia.

The second case is perhaps of greater interest because it points to the importance of the use of these solutions in cases of hemorrhage. A cervical cancer had been removed by a paravaginal operation and on the second day after operation the patient had a severe hemorrhage. The patient's condition was such that she rapidly became exsanguinated, and I was unable to attend her just at this time. Dr. Erlanger was communicated with and he felt it was a case for treatment with blood transfusion. According to the statement of the interne, a resident donor could be obtained within the period of half an hour. Unfortunately this interne was mistaken in his statement, because it was not until three hours later that a donor was found, and by that time the patient was in a dying condition.

The conclusion Dr. Erlanger draws in those cases where a blood transfusion is evidently necessary is that the gum-acacia-glucose solution injected at once will hold the patient until such time as a donor may be available and will only add to the probability of recovery.

DR. ROBERT T. FRANK, NEW YORK CITY.—My experience with gum-acacia-glucose solution was an acute one. It was in an evacuation hospital, where at one period of the conflict the patients came in with all degrees of shock, due to all possible reasons for producing shock. I include cold, exposure, hemorrhage, infection, starvation, dehydration, and so forth. We at first treated them with salt solution. I am leaving out the other treatments of shock, only referring to those which are distinctly applicable to this discussion. An order was later issued to use gum acacia with glucose. We were asked to employ quantities varying from 700 to 1000 c.c. Without having any time or opportunity for making a scientific investigation, a large number of men who were in charge of these cases separately came to the conclusion that harm was being done by these injections. We thereupon gradually reduced the amount of injection to a maximum of 400 c.c., and our results were better. However, we finally came to the conclusion that transfusion was far superior to any of the artificial sera which had been used. I exchanged opinions with other men from other organizations who arrived at the same conclusion.

DR. BENJAMIN P. WATSON, TORONTO, CANADA.—We have been using the gum-acacia solution now for nearly a year. We are fortunate in Toronto in having Dr. Keith who did a great deal of original work for the British Commission during the war, and we have come to replace normal salt solution entirely by gum-acacia solution.

Our experience with saline solution in shock and hemorrhage has been that you can raise the blood pressure at the time, and that blood pressure would be maintained for two or three hours, then there would be a sudden fall and a beginning state of shock again, requiring a later transfusion. We know that 25 per cent solution has been used by Dr. Erlanger, and with that percentage we find the blood pressure can be immediately raised. We had a case of ruptured ectopic pregnancy with the blood pressure down to 80 or 90 systolic and then brought it up to 120 systolic during the course of the operation, and that pressure was maintained.

We have had four cases where we followed the blood pressure right through a period of eight or ten days following operation and in none of these four cases did the blood pressure fall more than 6 points from what it originally reached at the end of the transfusion. To one we gave a second transfusion with the gum acacia. When this is combined with glucose in certain of these alarming cases, the effect is marvelous.

I will mention one severe case of hyperemesis. The patient came in in a condition of great collapse, a blood pressure of 90 systolic, and a low pulse pressure. She was very ill at the time. We gave her glucose solution slowly. She had it throughout the whole night. Her condition was so greatly improved, although vomiting continued, that we were able to empty the uterus. The patient ran a very long convalescence; she required repeated transfusions of glucose; she also had transfusions of blood. Ultimately she made a complete recovery and I am certain the patient never would have survived had we not had this means at hand for treating her.

One important thing in the gum-acacia solution is the method of preparation. A great many poor results have been due to bad preparations of the gum. We must use a pure preparation of the gum acacia, if possible. The solution must be carefully made. The water used must be freshly distilled water and the sterilizing of the solution in the autoclave must be carefully done. The solutions can be put up in greater strength and kept in the operating room and rapidly diluted at the time when they are being used.

I think it is most opportune that a subject like this has been brought before a large body of operating surgeons.

Dr. George Gray Ward, Jr., New York City.—The studies we have been making at the Woman's Hospital, as presented to you by Dr. Farrar, have been carried out with difficulty. Dr. Farrar has worked under the greatest handicaps. We found at first that we could not rely on the CO_2 estimations with the Van Slyke method in our laboratory in spite of having a full-time laboratory man available, because of the great amount of work called for by the routine work of the hospital. For that reason, we obtained a technician from the Rockefeller Institute who made these investigations so that they would be of value.

The gum-acacia solution, when we first tried it, was a failure from the fact that it was not properly prepared. It was not until we went to Toronto and saw it used there and got from Dr. Watson the exact technic in the preparation, which requires considerable care and many filtrations, that we had a satisfactory solution. There is no question in my mind as to the value and clinical effect of this method in cases of severe operation. The patients show remarkably little reaction compared to the other cases, notably in those cases Dr. Farrar spoke of in which a Wertheim operation was done and attended with considerable shock. One of the important things one must understand is that the administration intravenously of gum acacia is given throughout the operation by a slow method, as pointed out by Dr. Woodyatt, who has devised a special apparatus. Unfortunately this costs three or four hundred dollars and we could not get one, but we regulated the flow through needles and stopcock as well as possible, giving a slow continuous administration from the commencement of the operation. I think it might make a difference in the results if the solution were given very rapidly as one would give ordinary saline infusion. Perhaps that may account for the differences Dr. Frank observed in the army where they did not have the time for proper administration, but gave it as they would give the ordinary infusion. The high percentages used in the army may have made a considerable difference also.

The combined use of gum acacia and glucose, as suggested by Erlanger, is theoretically correct, and from our cases it would seem to be borne out clinically. Our results confirm the findings of Cannon that the blood pressure is a valuable index as to the CO_2 content, as regards acidosis. Instead of it being necessary to use the Van Slyke method, if we take the blood pressure throughout the operation, we have all the time an index as to the degree of acidosis, which greatly simplifies the practical study of the problem. We used a special stethoscope for that purpose and had readings made during these operations.

A point that Dr. Woodyatt made is very important, namely, that glucose energizes the muscle so that the muscle becomes tonic, as it were. This I think is shown in the rather free passage of flatus after the operation in these cases, from stimulation of the intestinal muscle, and there is also in addition quite a marked diuresis, as one would expect.

I wish to confirm the point made by Dr. Watson in severe vomiting. We had that borne out in our cases, where we could not relieve persistent vomiting until we gave glucose intravenously.

Dr. George Gellhorn, St. Louis, Missouri.—Is it possible to increase before operation the alkaline reserve of the organism to such an extent as to prevent acidosis?

Dr. J. Wesley Bovée, Washington, D. C.—I wish Dr. Farrar would give us more definite information as to the rapidity of the current in this slow process.

Dr. Joseph B. DeLee, Chicago.—How often do rigors occur after these administrations?

Dr. George W. Kosmak, New York City.—The method Dr. Farrar has described is undoubtedly a valuable one, but as a matter of practical application, would it be possible to substitute some other point of entry for the glucose?

I have for several years given glucose solutions (5 per cent) by rectum after operation in doses from 6 to 8 ounces at intervals of two or three hours and consider that I have had better results than with the use of salt solution for that purpose.

I would like to know whether in ordinary practice we cannot substitute the rectal administration of the glucose and whether it would not be of the same value?

Dr. Farrar (closing the discussion).—First of all, I want to thank Dr. Woodyatt for coming here and discussing this subject because it is really the physiologist upon whom we must depend to know what to put in a patient's blood.

Dr. Frank spoke about the results obtained in the army in giving gum acacia intravenously. One must find out exactly what can be put in the blood before one can obtain good results and it is only with the help of the physiologist that this can be done.

One of the speakers said that he had used a 6 per cent solution of glucose and that he had had only an occasional favorable result with it. I have kept to the 20 per cent glucose because of Dr. Woodyatt's findings, and have been guided entirely in glucose feedings by his work in the laboratory.

It was found by a special investigation committee in Great Britain, which did so much in trying out salt solution in hypertonic strength, and bicarbonate of soda which Cannon advocated, that dilution of the blood was not a good thing and if long continued was even harmful and that there was no especial advantage to be gained by it.

The thing of real importance was to get a solution which corresponded to the viscosity of the blood and that is what 20 per cent glucose and 5 per cent gum acacia does.

That brings up the question of rigors and chills. We have not seen any in this series. Two women who were given glucose intravenously in the ward for acidosis by an interne who did not appreciate the necessity of keeping to the rate although he had been told, did have chills but these were not operative cases. There were absolutely no chills or rigors in the other cases. There has been absolutely no case of infection or necrosis of tissue. We have had no bad results of any description.

As to the question of putting up the solution in a higher strength and diluting it, a point brought up by Dr. Watson, we find it more convenient to put the solution in flasks of 250 c.c., that means 50 grams to the flask, which is what we can give the average patient in an operation of one hour.

The technic is simple. An apparatus such as is used for giving salvarsan is prepared. The stopcock on the tubing regulates the rate of flow and an infusion thermometer in the tubing indicates the temperature, which should be 105° F.

With reference to the question of absorption of the solution by rectum, glucose is not absorbed by the rectum faster than 1.8 gms. per hour. The great value of glucose

lies in its introduction into the blood. It is not altered by the liver, it goes into the tissues and it has a far greater effect than glucose put into the stomach or bowel.

As to the prevention of acidosis, which was spoken of by Dr. Gellhorn, I could not take it up in the limited time at my disposal. It is possible to do a great deal towards prevention of acidosis by proper feeding of the patient beforehand. We can bring the alkali reserve up by giving the patient a protein-free, carbohydrate diet, and we can accomplish some good by the administration of bicarbonate of soda before the operation, but the great drop that occurs during the operation cannot be offset unless we do something at that time and the beneficial effect is then directly upon the tissues themselves just as Dr. Woodyatt has said.

DR. THOMAS WATTS EDEN, of London, England, a guest of the Society, presented a lantern slide demonstration of certain **Transition Stages from Benign to Malignant Conditions in the Ovary, the Tubes and the Vulva.** (The explanatory text and the descriptions of the more important sections shown by Dr. Eden, will be found on page 11.)

DISCUSSION

DR. ROBERT T. FRANK, NEW YORK CITY.—Dr. Eden has given us a very beautiful exposition of the transitional stages from nonmalignant to malignant conditions which can be observed in the ovary, in the uterus, cervix, and vulva. If to these he had added the inflammatory conditions of the tube with carcinoma following, and the sequence which we see in pregnancy, normal placenta, hydatid mole, chorio-epithelioma, he would have given us an absolutely complete view of the difficulties which confront the pathologist and secondarily also the clinician.

I cannot differ in a single word with what he has placed before you. His position is in marked contrast with that of a gentleman who recently published an article on the precancerous uterus in which, for instance, he quotes a case of chronic endometritis which he regards as adenomatous and says this woman, five years later developed cancer of the uterus. In a similar way he quotes a case of cervical polypi, followed in eight years by carcinoma.[1] If one is willing to extend the term precancerous to a period of time such as that, I am unable to follow either in conception or in argument.

As Dr. Eden has said, leucoplakia of the vulva or vagina is distinctly precancerous. Whether erosion can be explained from an inflammatory point of view or should be regarded as an adenoma, or at times be classified as a precancerous condition, I am unwilling to say. But the question I desire to ask most emphatically is this: Are we going to do more harm in operating upon the advice of a radical pathologist, or by watching and waiting if our clinical judgment in extremely doubtful cases restrains us? I am proud to say, in several instances I have been able to save the uterus and other parts of the genital tract from unnecessary mutilation where on slender evidence, such as Dr. Eden would reject, pathologists have advocated radical operations for epithelial overgrowth, for specific erosion, etc. I believe with an operative mortality from radical operation of at least 10 per cent or more, less harm will come in watching these cases with sane clinical judgment backed up thoroughly by good pathology.

The exposition you have heard of these transitional stages was most beautiful and illuminating.

DR. HOWARD C. TAYLOR, NEW YORK CITY.—This subject is rather difficult to discuss for one who is doing work largely clinical and comparatively little in the laboratory. When Dr. Eden left out the word precancerous, he took out most of my discussion.

A short time ago I had an opportunity to talk with a man who has one of the largest clinical laboratories in New York; a man who is doing extensive work for the best men,

[1] McCann, F. J.: The Precancerous Uterus, *Proc. Roy. Soc. Med.*, London, 1919, xiii. Sect. Obst. and Gynec., 3.

and who made the statement that physicians at the present time were relying too largely on clinical findings. In fact, he said, at times he was afraid to send out a report of a case in accordance with the findings, and he called attention to the question of precancerous lesions.

In talking with Dr. Stone who read a paper two or three years ago before this Society on precancerous lesions, he stated he had come in contact with a man who, as a result of reading his paper, was doing hysterectomy on account of irritated cervices, with the idea that it was a precancerous lesion. The head of the laboratory in New York made that same point; that if he turned out a report that a certain condition showed a precancerous lesion to a certain type of man, that man would be apt to do an extensive operation on that diagnosis which, of course was the right thing for the patient, but was not intended by the laboratory worker.

We all know how discouraging the cancer problem is. If a case of cancer comes into our hands early, there is no doubt a great deal of good work can be done by the proper treatment of these so-called precancerous conditions.

Precancerous is an old word which has recently crept into clinical work and is much misunderstood. As it is used and intended by Dr. Eden, it is different from the meaning applied to it clinically.

As Dr. Eden has said, so far as the ovary is concerned, very little can be done. When we make our diagnosis it means ordinarily that the organ has been removed.

I think the word precancerous as it is used by some laboratory men, brings out the fact that it is a condition which is somewhat in dispute. One pathologist will say that the lesion is benign and another will say it is malignant; in other words, it is a sort of intermediate stage between the two. With a condition of that sort it is difficult for the clinician to know what to do. As a matter of fact, we are prompted to decide very largely on the clinical findings influenced by the age of the patient and by her symptoms rather than on the pathologic report.

So far as the cervix is concerned, I think it is undoubtedly true as Dr. Eden has stated, it is the lacerated cervix with erosion in the multiparous woman rather than in a nulliparous woman which is the cause of the trouble. That, however, is a very close decision to make in the case of a woman who is at or near the cancer age, and it seems to me, the only practical way of treating these cases is to go on the basis that any lesion in that organ or neighborhood should be healed, and that ordinarily means removal by operation rather than by applications of caustics or cauterization by heat.

Dr. Frederick J. Taussig, St. Louis, Missouri.—I wish to confine my remarks solely to the question of precancerous conditions of the vulva. Some four years ago I presented before this Society a short report upon an etiologic study of vulvar cancer which dealt in some degree with this question. Since that time I have had occasion to have quite a considerable amount of additional material come to me for microscopic study, and I have recently made a report of 20 cases of leucoplakic vulvitis and 27 cases of vulvar carcinoma from which I believe certain deductions can be drawn. Prior to this study I made an analysis of 100 cases of normal involution of the vulva in old women. Rather to my surprise, I found that in about 20 per cent of these old women, with an average age of 65, there was a complete obliteration of the labial folds and the clitoris was practically absent; therefore, the emphasis that has been laid upon such an obliteration in the past in the term "kraurosis" is not to my mind justified. Berkeley and Bonney have been working on this subject and their views are extensively presented in Dr. Eden's very interesting and complete three volume work on gynecology. These men have really gone into the subject more completely than any other previous investigators, and their views are practically identical with what I have found. Kraurosis is a different thing from leucoplacic vulvitis. It is an obliteration of the labial folds and clitoris with at times some abrasions, but not having that essential histologic factor, namely, the absence of elastic tissue in the dermis. Leucoplakic vulvitis is an interesting and unusual histopathologic condition, for besides the changes mentioned in the epidermis, by Dr. Eden, I believe even more important are the changes in the dermis itself. In none of the cases

I have studied was the absence of elastic tissue from the upper layers of the dermis noted. Dr. Engman, our dermatologist, is also convinced that this absence of elastic tissue was the primary factor in the condition, probably in some degree secondary to the absence of corpus luteum, because all these cases, or practically all of them, were in women past the menopause. As a result of the absence of elastic tissue there occurred a greater friability of the tissues, an increased tendency to slight breaks, and, because the epidermis cracks, the infecting organisms connected with the infection gain entrance from without and a pruritus begins. Thus the process grows worse; the epidermis becomes thickened, we see a picture such as Dr. Eden showed in one of his slides, with prolongations, and in that stage the tendency to carcinoma is, of course, greater than in any other. I do not think, however, that Dr. Eden is justified in assuming that the last picture shown, where there was a glairy appearance of the dermis, is a stage of healing. I presented before this Society four years ago a similar slide of a section where the epithelium was thinned out but the dermis showed that malignancy was developing. Therefore, such malignancy can occur in this later stage, although not as frequently as in the proliferating stage.

DR. JOHN G. CLARK, PHILADELPHIA.—A point which has been brought out in this demonstration is the necessity primarily for controlling the microscopic diagnosis by clinical judgment.

I have been particularly impressed recently with the attitude of Dr. Ewing, of New York, in this matter. Dr. Ewing is one of the most level-headed pathologists we have in this country. He takes the position that in a questionable case a man is operating against clinical judgment; he is not willing to declare in a questionable case whether the disease is or is not malignant. He made the statement that a clear, nonmalignant history was a good thing and would go far as a final factor in diagnosis. For that reason, two things are paramount. First, a history of the case, and secondly a pathologist who controls our ultimate clinical work. In this respect our attitude should be more or less the attitude of a special society whose members can finally determine when or when not to operate much better than the general practitioner can. On the same principle I believe every good laboratory should be controlled; that every good operating room should be controlled by a special pathologist, for the reason that frequently we see a difference in diagnosis between the best general pathologists. For example, a pathologist in the University of Pennsylvania, in a case with a perfectly clear nonmalignant history, gave the opinion that the disease was positively carcinoma, although the clinical evidence was against it, and when the question was put up to us we simply said, we will not operate. The subsequent history of the case showed positively that our clinical judgment was accurate and not the judgment of the pathologist.

DR. FRED L. ADAIR, MINNEAPOLIS, MINNESOTA.—Some years ago I spent considerable time in going over a large number of cervices, particularly in relation to erosions around the external os. Aside from the so-called congenital erosion, which has no particular place in this discussion, the erosions seem to originate from a combination of inflammation, maceration and traumatism. It seems to me, that this point is of importance in connection with the subsequent history of some of these cases of erosion. Most of these erosions develop in association with parturitio, or in some cases from inflammatory conditions which involve the cervix. In order to avoid the development of these erosions into malignancy, early treatment should be instituted. This can be done by proper supervision of the obstetrical cases and the early treatment of these erosions by the elimination of those different factors which enter into the individual cases, namely, the traumatism, inflammatory processes, and the maceration. The columnar epithelium is better able to resist these factors than the squamous epithelium, particularly maceration, and that is the reason erosions occur. After some of these factors have subsided the squamous epithelium seems to have greater resistance and is better able to survive, I was able to demonstrate definitely in some specimens that in the process of healing there is no metaplasia or transition of the columnar epithelium into squamous epithelium, or of the latter into columnar epithelium. This can be distinctly demonstrated by differential staining with mucocarmin, showing squamous epithelium growing underneath the columnar. This takes place not only on the surface,

but it grows down into the glands underneath the columnar epithelium. In some instances I was able to show complete Nabothian cysts which were lined with squamous epithelium, in which the columnar epithelium was either partially or completely replaced by squamous epithelium, not by metaplasia but by definite undergrowth of squamous epithelium. That metaplasia can occur I am not prepared to dispute, but that it is usual in the process of healing in this condition, I do not think is true.

These erosions present a most fascinating picture because of the varying proportions of epithelium and glands. This comprises a very fruitful field for studying the transitional changes between the ordinary simple erosion and malignancy.

DR. JOSEPH BRETTAUER, NEW YORK CITY.—From a clinical point of view, we all subscribe to what Dr. Clark has just said. "Cancerous age" as a specific age ought not in my opinion be used. There is no such age. We see cancers from the age of fifteen up to the postclimacteric period.

DR. JOSEPH B. DE LEE, CHICAGO.—No one has said anything on the obstetric side of these conditions that have been demonstrated on the screen. I am prompted to say that if obstetricians devoted more care to their obstetric cases they would not see so many lacerations and erosions which may or may not ultimately develop into cancer. If practitioners will observe certain rules there will be fewer lacerations and erosions, especially not to dilate the cervix with the fingers when they do not have to. Second, not to give pituitrin. Third, not to use bags. Fourth, not to infect the woman, and fifth, if lacerations should occur, repair them at once.

DR. GEORGE GELLHORN, ST. LOUIS.—In general surgery and in dermatology the actual transformation of syphilis into cancer has been a long established fact, but in gynecology such a transformation is thus far only a matter of surmise.

I would like to show three slides which may throw some light upon the histogenesis of genital cancer upon the basis of syphilis.

Fig. 1

The first picture (Fig. 1) is from an old syphilitic ulcer of the vestibule described in Dr. Ehrenfest's and my report before this Society four years ago. On the right side there is normal pavement epithelium; underneath a zone of infiltration of lymphocytes and plasma cells which may be noted as gradually thickening toward the middle of the picture, with enormous proliferation of blood vessels with thickened intima. On the left side there is a curious proliferation of pavement epithelium which sends out long slender processes into the underlying tissue, a picture suggestive of cancer were it not for the fact that these processes do not extend beyond the area of lymphocytic infiltration. Furthermore, the basal layer of this epithelium is perfectly normal and the basal membrane is nowhere broken through. The case was subjected to treatment and a few weeks later, when a second excision was made, the epithelium showed normal characteristics.

Figs. 2 and 3 were taken from a paper by Rohrbach, dealing with a case of an old luetic ulceration of the labium majus. Here there is also a very large proliferation of pavement epithelium, and in the area of lymphocytic infiltration there is an isolated nest of epithelial cells which, on serial sections, was found not to be connected with the surface epithelium (*x* in Fig. 2).

Fig. 3 shows an entirely different aspect of the same case. The processes of epithelial cells extend into the underlying stratum and the normal epithelium is greatly altered. The basal membrane is broken through. The cell contours are no longer clearly defined. The nuclei have lost their staining qualities and are vesicular.

This, then, is a picture which corresponds in all details with that given by Ribbert of the earliest stages of epidermal cancer. The reason why Rohrbach's diagnosis was the same as in my first case was that the proliferation did not extend beyond the zone of lymphocytic infiltration into preformed tissue. This case was also subjected to energetic treatment, and within a short time, the pathologic changes had disappeared. The fact that many of these

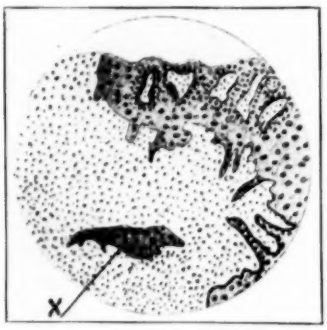

Fig. 2

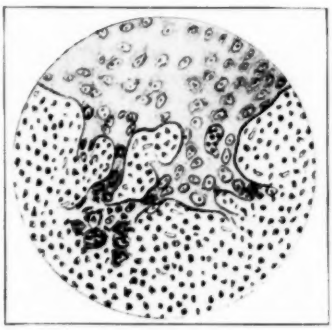

Fig. 3

cases are subjected to treatment seems to prevent the actual transformation of syphilis into cancer. If, however, such cases are neglected, there is every reason to assume that in a fairly large proportion of them the actual transformation of syphilitic into carcinomatous tissue takes place.

DR. EDEN (closing the discussion).—I was much interested in Dr. Adair's remarks. I did say in describing erosion, it was formed by the stratified epithelium giving way to the columnar epithelium, not a metaplasia. I said there was only one sort of metaplasia so described, and that was one in which some day we would be dealing with a healing erosion or early cancer.

I am very much in sympathy with Dr. Clark's position. I suppose all of us have suffered at the hands of the general pathologist. There is no doubt that for gynecologic pathology a man needs special training. I was especially glad he pointed out the importance of keeping the laboratory work and clinical work in close association.

I feel I really ought to apologize for talking to an audience of clinical men about the pathologic side, but I do think that men who operate ought to know what they see when they look down a microscope.

I have not given you any abstruse details of histology, but such points as we have been discussing we ought to be in a position to settle ourselves. While we cannot escape seeking the help of the pathologist from time to time, I think clinical gynecologists who run large services ought to be able to do most of the histology themselves.

(To be continued in November issue.)

Department of Reviews and Abstracts

CONDUCTED BY HUGO EHRENFEST, M.D., ASSOCIATE EDITOR

Collective Review

The Prophylaxis and Treatment of Puerperal Infections

(A Critical Review of Recent Literature)*

BY HUGO EHRENFEST, M.D., F.A.C.S., ST. LOUIS, MO.

The importance of the problem of puerperal sepsis is succinctly stated by Krusen[1]: While in the opinion of the medical profession and of the laity the number of deaths from puerperal septicemia is fast decreasing, many eminent obstetricians have shown that outside of well-managed hospitals there actually has been no great reduction in totals or in the rate of deaths. It is high time that obstetricians teach, by precept and example, the gospel of puerperal complications. DeLee, never tiring in his efforts to bring the profession to a proper realization of the seriousness of the situation, says in a discussion of everyday obstetric problems: "Taking everything into consideration I feel sure that the statement cannot be contested that 8000 women die annually in this country from puerperal infections." The same discouraging situation apparently also exists in Europe. Quoting an English writer, Brock[3]: "If results are properly gauged by accurate figures, as those offered in the British health statistics, then it must be admitted that the mortality rate from puerperal sepsis shows only a small reduction. A study of these returns for the past seventy years reveals a death rate for some of the earlier years almost identical with that of some recent years." Grace L. Meigs[4] made for the Children's Bureau of the U. S. Department of Labor a thorough study of maternal mortality from all conditions connected with childbirth in the United States and certain other countries. In the summary we find among others the following statements: "In 1913 in this country at least 15,000 women died from conditions caused by childbirth; about 7000 of these died from childbed fever, a disease proved to be almost preventable, and the remaining 8000 from diseases now known to be to a great extent preventable or curable. Physicians and statisticians agree that these figures are a great underestimate. In 1913 childbirth caused more deaths among women fifteen to forty-four years old than any disease except tuberculosis."

"Only two of a group of fifteen important foreign countries show higher rates from this cause than the rate in the registration area of the United States. The rates of three countries, Sweden, Norway, and Italy, which are notably low, prove that low rates for these diseases are attainable. The death rates from childbirth and from childbed fever in this country apparently are not falling to any great extent. Indeed, during the thirteen years from 1900 to 1913 they have shown no demonstrable decrease."

"These facts point to the need of higher standards of care for women at the time of childbirth. Improvement will come about only through a general realization of the necessity for better care at childbirth. If women will demand better care, physicians will provide it, medical colleges will provide better

*Elaborated from a paper read at the Meeting of the American Gynecologic Society, Chicago, May 24-26, 1920.

training in obstetrics, and communities will realize the vital importance of community measures to insure good care for all classes of women."

Reading these lines one cannot suppress a feeling of wonder why the women of this country, who must be credited with most of the notable achievements in child welfare, have not yet awakened to the import of this serious problem of their own welfare. It does not seem fair that the medical profession, by assiduous and effective propaganda, has familiarized them with the perils of cancer, but at the same time has concealed from them the greater risks of childbirth. It is a disingenuous procedure to extol the attainments of modern surgery and to hide the lamentable deficiencies, not of the science, but of the everyday practice of obstetrics.

PROPHYLAXIS

Is puerperal sepsis an entirely preventable disease? Probably not. Zangemeister and Kirstein[5] a few years ago advanced proof that the vagina contains saprophytes which occasionally ascend into the uterus. Thus nobody directly connected with the delivery may be actually responsible for the infection. Loeser[6] more recently investigated most thoroughly the problem of the latent infection of the birth canal. Much work has been done in the past to study the invasive faculties of all vaginal bacteria, including the anaerobes, always with particular consideration of the streptococci. The results of these various investigations, in general, concur in the fact, that but few of the bacteria of the vaginal flora are true saprophytes, that the majority of them, under favorable conditions, might exhibit pathogenic characteristics. Loeser assumes that in the genital tract of women during the reproductive age conditions change so often and so greatly, that conditions favorable for the development of pathogenic characteristics by some of the vaginal microorganisms are likely to prevail at some time or another. Therefore, it is justifiable, and possibly preferable, to admit the existence during pregnancy of a state of latent infection. This assumption obviously does not exclude the possibility of an added exogenous infection as the result of careless manipulations, etc. The traumatisms of labor, open wounds, presence of blood, etc., represent circumstances, most propitious for bacterial growth. If in the vagina, where, as a rule, innumerable varieties of bacteria live as saprophytes in harmonious symbiosis, one particular bacterium is found in pure culture, the deduction seems justifiable, that conditions prevail which facilitate the development of this one form at the expense of all others. Loeser feels that the assumption is warranted, that these also might be the conditions under which this particularly favored bacterium also develops virulency, or an increased virulency. In this manner it might be explained that latent microbes, and not by any means only streptococci, will suddenly assume the characteristics of aggressiveness. In regard to streptococci in particular, it seems probable that the change of the chemic reaction of the vaginal discharge from the normally acid to alkaline after labor, enables them to manifest their innate quality of invasibility which is inhibited, as well known, by growth in acid media. Only bacteria which are pathogenic or become so under certain conditions are capable of provoking the production of defensive antibodies in the invaded human organism. Therefore, the presence of agglutinating substances proves the pathogenic character of the bacterium, or indicates a state of latent virulency if the characteristic clinical symptoms of an infective process are absent.

Another type of puerperal infection, not entirely preventable, but fortunately rare, is that due to the tetanus bacillus. Spiegel,[7] from an analysis of 66 authentic cases of puerperal tetanus, recorded in literature, concludes that the tetanus bacilli enter the circulation either from the endometrium or from perineal wounds. The prognosis of this disease is entirely dependent upon

its early recognition. Immediate institution of the serum treatment yields most satisfactory results, especially in the more chronic cases in which the symptoms develop gradually until about the fourth day the first typical severe convulsion occurs.

In the light of recent advance of our knowledge concerning focal infections their possible relation to puerperal fever is worthy of careful study. This problem is discussed by Talbot.[8] Although he argues only theoretically on the problem, without advancing a single convincing proof, one must agree with his general conclusion, that from a prophylactic point of view it is incumbent upon the obstetrician to examine every pregnant woman to ascertain whether such a focus exists, and to have it removed before it possibly could do harm. In a similar manner, Davis[9] refers to the intestinal tract as a potential source or seat of an infectious process that might complicate pregnancy or the puerperal state.

Contradictory views will be found in literature (and textbooks) concerning the importance of the duration of labor after rupture of the membranes in the etiology of endogenous puerperal infections. This problem is the subject of a very comprehensive paper contributed by Rohde.[10] He concludes that both in women who have, and those who have not, streptococci in the vagina, the duration of labor after the escape of the amniotic fluid does not play a rôle worthy of consideration in the causation of fever, during and after labor, and in puerperal mortality. Rohde presents so many carefully arranged tables covering several thousands of cases, on which he bases his conclusions, that it seems fair to assume that he has definitely settled this question.

Another most noteworthy fact is recorded in a paper by Slemons.[11] Extensive statistics indicate that in from 2 to 3 per cent of cases a rise of temperature above 101° F. can be noticed during labor. In severer cases, after delivery the fever continues. There has never been any doubt that in this group the intrapartum complication is dependent upon a bacterial infection.

In the other group of milder types, probably in the majority of cases, the rise of temperature passes unnoticed. As a rule, the fever is ascribed to the effect of prolonged and violent uterine contractions. Warnekros (in 1913) established the fact that also in these cases the rise of temperature is of bacterial origin. Out of a total of 25 febrile women in labor he obtained positive blood cultures in eighteen.

Slemons extended the search for the source of this infection to the placenta which he studied in stained sections. Most conclusive evidence was thus obtained for two facts: First, that intrapartum fever, unless attributable to some accidental cause such as tuberculosis, is due to a placental bacteriemia, secondly, that this infection does not proceed from the maternal circulation into the villi, but vice versa. The bacteria evidently enter the placenta by way of the amniotic membrane and the amniotic fluid. Generally the latter becomes infected, because the membranes had ruptured prematurely (which is not in accord with the findings of Rohde quoted previously), labor prolonged, and repeated vaginal examinations had been made. Since in these cases the placental infection usually is limited to the amniotic surface of the organ, the complication is likely to prove more serious to the fetus than the mother. It probably often is responsible for the fetal death.

Hematogenetic contamination of the amniotic fluid may occur in any case of maternal septicemia, as has been emphasized by DeLee and also Curtis, but more common is the ascending type of infection.

In regard to their prognostic value, Slemons expresses the opinion that positive cultures, obtained during labor, are of no value, while in the puerperal state negative cultures signify a more favorable prognosis than positive ones.

Brock,[3] already quoted, like Slemons emphasizes the great advantage of

avoiding frequent internal examinations. Careful prenatal study of the patient makes this possible. It might be mentioned in this connection that the German literature of the war years contains many articles praising the superiority of external abdominal, and rectal over vaginal examinations during labor. The absolute lack of rubber gloves forced German and Austrian physicians to find a substitute for vaginal explorations.

Other necessary changes in the customary obstetric technic in the interest of a better puerperal prophylaxis are advocated by Berry Hart.[12] Much more care should be taken in preventing perineal lacerations. Crede's method of expression of the placenta, in his belief, is a very serious error in the management of labor. It tends to leave pieces of tissue attached to the uterine wall, because the placenta is forcibly removed before it had become completely separated.

The very satisfactory results of prophylactic, active immunization with specific sera and vaccines in many of the acute infectious diseases prove the feasibility of similar efforts to obtain effective immunization against streptococcic puerperal sepsis. Joetten[13] discusses the literature on this subject, and acknowledges the final failures of all previous attempts. He prepared a vaccine from six different strains of streptococci cultured from septic puerperæ. Experimenting with a gradually increasing dose of from 25, 50, 100, 250 up to 500 millions of bacteria to one c.c., he observed a gradual and proportionate decrease in puerperal febrility. In 819 cases, injected prophylactically only with 25 to 50 millions of bacteria, the percentage of febrility was 16 per cent in the last series of 126, with a dose of 500 millions of bacteria, it had been reduced to 7.1 per cent. Also the cases of streptococcic death seemed to decline in a similar ratio. Of the 819 cases of the first group four died, of all the remaining cases, treated with a dose above 50 millions of bacteria, only one died of a streptococcus sepsis. Joetten does not wish to exaggerate the meaning of these observations, but feels that they at least leave hope that successful protection against streptococcal puerperal sepsis might be obtained by an improved method. Garcia[14] claims good results from a routine injection of 20 to 40 c.c. of antistreptococcus serum in every woman entering the maternity in labor.

THERAPY

The question of the value of such surgical procedures as hysterectomy, ligation or resection of veins, etc., is still under discussion, but a marked trend toward conservatism can easily be noticed.

From countries far apart, from France, Australia and South America, come expressions of more or less enthusiastic approval of operative interference. Cadenat[15] feels sure that many more women, suffering from puerperal sepsis, could be saved by a vaginal hysterectomy, newly devised by him. It can be performed in from three to fifteen minutes, avoids contamination of the peritoneum and insures a wide channel for drainage. He considers this operation indicated in every case, in which no appreciable improvement within 24 hours is seen after thorough curettage of the uterus—and it may be emphasized here, that this is the only paper, which the reviewer has seen in recent literature, in which a writer mentions the curette without condemning its use in the puerperal septic uterus. An article of Nyulasy[16] does not specifically mention the extent of the author's personal experience with excision or ligation of the infected pelvic veins, but he is convinced that early operation may save many cases which are lost when treated on the lines hitherto followed. "Early operation to me has become a supreme duty." Turenne[17] thinks that, contrary to generally expressed opinion, puerperal, septic, utero-pelvic thrombophlebitis has signs, symptoms, and a clinical evolution which permits a diagnosis in the majority of cases. Although in more than half of these cases there

is a tendency toward subsidence and recovery, the high mortality of the remaining justifies modern operative methods of treatment. Surgical intervention, especially ligation of the thrombosed veins, is rational. Ligation of all the efferent venous trunks of the genital zone is desirable. Operations on veins are contraindicated by a permanent bacteremia, accessible thrombosis and in cases of visceral pyemic localization.

It proves much easier to quote papers, based on thorough anatomic investigations or on careful analysis of a very large clinical material, which discourage surgical treatment in general or condemn certain operations.

Sampson[18] shows by x-ray studies, that foreign material can be easily forced from the uterine cavity into the uterine veins, if the endometrium is injured or has been removed by curettage. He draws the most plausible conclusion that uterine contractions following relaxation when the cervical canal is obstructed, and intrauterine douches supply sufficient pressure to effect this escape of material from the interior of the uterus into its veins. This surely explains one way by which a general puerperal infection may result from intrauterine manipulations. Halban and Koehler,[19] in a comprehensive monograph published in book form, consider a very extensive material studied at autopsies. Analyzing the anatomic findings from the viewpoint of excision or ligation of pelvic veins, the surgical treatment of peritonitis, and hysterectomy, they feel that these findings clearly speak against the possible usefulness of all such procedures. Therefore, it cannot be surprising that surgical therapy of puerperal sepsis has rather generally proved a failure in practice. They are inclined to believe that in some of the operated cases seen on the postmortem table the operation possibly had removed the chances of spontaneous recovery. Conditions seem slightly more favorable for surgical intervention in peritonitis. Halban and Koehler thus arrive at the final conclusion that hope for better results does not lie in surgery but in an improved specific antibacterial treatment, or possibly in the discovery of a specific chemic bactericide of the type developed by Ehrlich.

A critical survey of the various methods of treating pelvic infections by Polak[20] begins with the statement that puerperal infections are directly proportionate to the number of vaginal examinations, therefore, abdominal and rectal examinations are preferable. Curative treatment is based on the proper recognition of the natural pathology. Realizing that the interior of the uterus is the principal portal of entry for bacterial infections, the fruitlessness and fallacy of all intrauterine manipulations, of curettage or irrigation, must be obvious. Drainage is more effectively obtained in the Fowler position, supplemented by having the patient lie on her abdomen. Ergot, pituitrin and ice-bag stimulate contraction and retraction of the uterus. After bacteria once have begun to pass from the uterus into the myometrium, or into lymph and blood-vessels, intrauterine manipulations can do only harm. In parametrial infections he considers absolute conservatism the best treatment. In beginning peritoneal invasion he follows an expectant therapy. Only if there is evidence of extension, the posterior culdesac may be opened. General hygienic and dietetic measures will help the patient in the fight against the bacteremia. Vaccines and sera have not proved of advantage. In cases of general peritonitis, incision, drainage, etc., have given no better results than the expectant plan.

The entire therapy of puerperal fever is still more exhaustively discussed by Schaefer.[21] Every form of local treatment, either for puerperal ulcer or septic endometritis has been definitely discarded. They never do any good, but are likely to do harm. The antiseptic vaginal douche is useless, but at least less dangerous than the intrauterine douche which may be directly responsible for an acute pyosalpinx or a general peritonitis. Any attempt to remove the septic endometrium with curette or spoon is extremely objectionable. There

are at present only two indications for local treatment left: (1) In cases of obvious retention of lochial secretions, in which the rise of temperature coincides with the sudden stoppage of all lochial flow, a glass tube may be carefully introduced into the uterus; (2) larger pieces of necrotic placenta may be removed, but only with the finger, smaller fragments are better let alone.

Extirpation of the infected uterus, as a rule, proves futile, with the sole exception of a postpartum necrosis of a fibroid. Surgical interference in cases of puerperal infection in the Berlin Frauenklinik at present is limited to: (A) Opening of pelvic cellular abscesses. If the tube contains pus, conservatism, rest and opium are preferable for immediate treatment. (B) In cases of peritonitis two flank incisions, without subsequent lavage, are made, preferably under local anesthesia, as soon as the diagnosis is positively established by the aspiration of pus through a hypodermic needle. (C) In cases of thrombophlebitis, ligation gives satisfactory results only in chronic cases, in which the process has persisted for several weeks, has remained limited to the veins, and has not caused phlegmonous processes in the uterus or surrounding tissues. In all acute cases the thrombosis is very likely to progress beyond the ligation, and the operation then may become responsible for a parametrial abscess, if not a peritonitis.

But most important, continues Schaefer after having finished the consideration of surgical therapy, is the general treatment which should be instituted promptly in every febrile case, in an endeavor to localize the process and to help the organism to develop protective powers against the bacterial invasions. For the purpose of obtaining early localization the use of heat at present has completely replaced the ice-bag formerly employed. For the purpose of general protection it is important to rid the circulating blood, as promptly as possible, of the germs already entered, and to prevent further entrance. A clear picture of the situation can be obtained only by careful study of the blood, repeated at least every other day, always including a blood culture. Antistreptococcus serum has failed with few exceptions, also the various colloidal silver preparations. Trials with the administration of methylene blue by mouth proved futile. Then Bumm suggested the combined use of antistreptococcus serum and methylene blue. This new treatment has yielded satisfactory results when applied in the following manner: 50 to 60 c.c. of antistreptococcus serum are injected subcutaneously into the thigh, followed in one or two hours by the subcutaneous injection of 0.05 grams of methylene blue in 20 c.c. of physiologic salt solution. Under steady control of the blood these injections are repeated daily for several days. Experience gained in the past two and a half years, in the opinion of the writer, encourages a continuation of this medication.

Reference has been made to the hope expressed by Halban, that a useful general chemic bactericide for puerperal sepsis might be developed. With this problem deals a paper of Miller and Chalfont.[22] Many different solutions have been used intravenously in the treatment of puerperal bacteremia. But, as the writers find from a study of the literature, apparently not one has stood the test and none has been adopted for general use. Bleyne tried salvarsan, and suggested 30 centigrams as a safe dose, but Hussy warned against its employment on account of its toxicity. Miller and Chalfont recommend arsenobenzol in a dose of 6 milligrams given immediately without the delay of a blood culture when the case offers the clinical symptoms of a blood stream infection. A decided drop in the leucocytes without a corresponding drop in temperature and pulse rate within the next twenty-four hours was considered an indication for repeating the injection. Eleven patients in all so far have been treated in this manner. Five received only one injection, three had two, one had three, and the two remaining, four injections. In every case they succeeded in ridding the blood stream of the invading germs.

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Selected Abstracts

The Bacteriology and Chemistry of the Vagina

DeLee: Trichomonas Vaginalis Vaginitis. Illinois Medical Journal, 1920, xxxvii, 186.

DeLee calls attention to this type of obstinate vaginitis, which is not rare. It usually proves most refractory to the customary types of treatment, because it is not properly diagnosed. Its clinical symptoms are persistent, and the profuse vaginal discharge, causing much annoyance to the patients on account of burning and pruritus, by way of sleeplessness leads to a neurasthenic "run down" feeling. The discharge is acrid, of a disagreeable odor, very irritating, and often causes the formation of pointed condylomata. The peculiar, granular appearance of the vaginal mucosa, "rough like a nutmeg grater," usually with small hemorrhagic points, almost enables one to make the diagnosis, which, however, must be confirmed by a microscopic examination of fresh vaginal discharge for the characteristic Trichomonas vaginalis. The writer found the following treatment most effective: On the first day, vagina and vulva are most carefully scrubbed, vigorously, with tincture of green soap and water by means of a rough cloth. The soap is rinsed out with distilled water. The process is repeated three times, followed by a 1:1500 mercuric chlorid douche and finished with another douche of distilled water. The patient is kept in bed. Next morning, the vagina is again scrubbed with green soap and sterile water, and then packed with cotton soaked in glycerin (4 ounces) and sodium bicarbonate (1 ounce). The next morning, cotton is removed and sterile water douche is given. As a rule, on the next following day the microscope will prove the disappearance of the trichomonas.

Schroeder and Loeser: Trichomonas Colpitis. Monatsschrift für Geburtshülfe and Gynaekologie, 1919, xlix, 23.

Hoehne (in 1916), as the first, emphasized that Trichomonas vaginalis, generally considered a harmless parasite, is found in strikingly large numbers practically in all the severer and more obstinate types of vaginitis. He concluded that there might exist an etiologic relation. Later Kuestner, Traugott, and Wille confirmed Hoehne's observations, and also accepted his explanation. Wille stated that trichomonas can be detected in about 40 per cent of all free vaginal discharges. In a total of 2183 cases examined, Schroeder and Loeser, however, found only in 120 instances trichomonas. Investigating thoroughly the entire bacterial flora in these cases, and comparing their results with those of many previous investigators, they can only confirm the common assumption that most of the bacteria vegetating in the normal vagina originate

from the intestinal tract. The same holds true for the trichomonas, which by predilection seems to remain in the Lieberkuehn's glands of the intestine. They discovered that in these cases of colpitis, containing trichomonas in large numbers, the *Micrococcus gazogenes alcalescens* is rarely missing. This accounts for the foamy character of the secretion, mentioned by various writers as characteristic for the trichomonas vaginitis.

Analyzing carefully the bacteriology of the vagina under normal and pathologic conditions Schroeder and Loeser arrived at the final conclusion that the presence of an abnormal flora provides certain conditions which are particularly favorable for the propagation of the trichomonas. Not one of the many varieties of the trichomonas, so far known, has ever been found to possess pathogenic characteristics, therefore, it does not seem logical to assume that the Trichomonas vaginalis should be able to produce by itself a specific colpitis. The term trichomonas vaginitis is misleading and should be discarded. Of all the numerous methods of treatment for this disease, of which some have been designated as specific, the one will give temporary or permanent relief, which in the individual case is able to restore the vaginal flora approximately to normal. The trichomonas then is deprived of suitable conditions to thrive and disappears, coincident with the improvement of the vaginitis.

Noguchi and Kaliski: The Spirochetal Flora of the Normal Female Genitalia. Journal of Experimental Medicine, 1918, xxviii, 559.

Except for the occurrence of the well-known *Spironema refrigens* (Schaudinn and Hoffmann) in both male and female genitalia, practically nothing is known about the spiral organisms in the normal female genitalia. On anatomic and physiologic grounds the female genitals undoubtedly afford more favorable conditions than the male for the existence of these saprophytic spirochetes. The examinations of the smegma, films and washings of the genital mucous membranes of normal adult females actually demonstrated that the number of spirochetes in the female is much greater. The varieties are identical in both sexes: *Treponema calligyrum, Treponema minutum,* and *Spironema refrigens,* the first mentioned usually predominating.

Examination of the spirochetal flora of female children up to the age of two years showed conditions very similar to the adult.

Goodmann: The Effect of Weak Acetic Acid on Spirocheta Pallida. Journal American Medical Association, 1920, lxxiv, 803.

Experiments lead the writer to the conclusion that the *Spirocheta pallida* apparently is unable to live in an acid environment even as low as a 0.5 per cent solution of acetic acid. He suggests that an acid solution may prove of use in the prophylaxis of syphilis. He points in this connection to the fact that syphilitic chancres of the vagina are admittedly less frequent than chancres of the neighboring parts. The secretion of the vagina in adults is acid while that of the adjoining parts of the genital canal is alkaline in reaction. Certainly the vagina is equally subject to trauma and the deposition of the spirochete as the cervix, and the labia, the most common seat of the primary chancre. In the few reports of vaginal chancres which give the exact location of the primary lesion, it is most often found in the posterior fornix, i.e., where alkaline secretions coming from the cervical canal will neutralize the acid vaginal secretions.

Graefenberg: Cyclic Changes in the Acidity of Vaginal Secretions. Archiv für Gynækologie, 1918, cviii, 628.

It has been known for some time that the vaginal secretions show acid reaction in approximately 60 per cent of the nonpregnant, but in between 90 and 100 per cent of pregnant women. This difference in itself suggested

strongly the dependence of this acidity upon certain physiologic changes in the genital tract. Graefenberg assumed that these conditions could be understood better by determining, by means of titration, the quantitative average of the acid contents of the secretions. Finding the acidity variations far too great for the safe calculation of an average, even in apparently healthy women, he considered it more promising to divide them into groups which would comprise definite clinical conditions such as healthy nulliparæ, healthy multiparæ, patients with leucorrhea, pregnant women, etc. Again he discovered the same fluctuations within the same group. Then he began to determine the acidity of the vaginal discharge of the same woman at different times by repeated titrations. Once more he could observe striking changes, the index of acidity often multiplying many times within a few days. He started to plot these changes of the same individual in a graphic chart, and obtained curves revealing a certain regularity. When he recorded on the same chart also menstruation, he was finally enabled to establish the surprising, but very evident, fact that in all women the acidity and menstruation curves run parallel, the acidity curve reaching its peak just before menstruation and beginning its decline immediately after menstruation, the lowest level falling into the menstrual interval. Continuing these investigations on patients, in whom the uterus, but not both ovaries had been removed, on patients suffering from leucorrhea, and also on pregnant women, he came to the definite conclusion that this rhythmic change in the acidity of the vaginal secretions is not dependent upon any function of the uterus, or the admixture of menstrual blood, but must be directly caused by that part of ovarian function to which today is generally ascribed the wave-like, rhythmic change of many other conditions (blood pressure, pulse rate, temperature, metabolism, etc.) during the reproductive age of woman. How closely the acidity variations are associated with the evidences of sexual maturity Graefenberg was able to demonstrate by the acidity curve of women entering the menopause, when the wave peak becomes lower and lower, and the curve actually changes into an almost straight line, gradually declining until it reaches zero. If at the beginning of menopause menstrual flows become irregular, the acidity curve still maintains its regularity, showing strikingly that acidity is independent from uterine activity and entirely determined by ovarian function.

The acidity of vaginal discharges cannot any longer be explained as due to the Doederlein bacillus, a fact which indeed has been doubted by previous investigators. This change in acidity presumably is significant in the reproductive process, the lowest acidity, i.e., a condition more favorable for the spermatozoids, prevailing at a time when according to recent investigations conditions in general have been found most favorable for fruitful cohabitation. The variations in vaginal acidity are but a part of those cyclic changes which express normal ovarian activity, and, therefore, it cannot be surprising that acid values are found relatively low and less fluctuating, much like in the menopause, also in those cases of primary sterility in which certain clinical and anatomic findings suggest ovarian hypofunction as the underlying cause.

Engelhorn: Biology of the Vagina. Monatsschrift für Geburtshülfe und Gynaekologie, 1919, 1, 282.

Titration of the vaginal secretions of pregnant women shows a higher acidity than in the nonpregnant. This may be explained as due to certain histologic changes (edema, dilatation of vessels, hypertrophy of papillæ, etc.) In the same manner histologic changes probably also account for the evident reduction in acidity during the menopause. In the presence of an alkaline

reaction in the vagina the introduction of sugar will bring a prompt change to an acid reaction.

Stellwagen and Pelouzi: Are Diphtheroids a Factor in Female Sterility? Journal American Medical Association, 1918, lxx, 977.

This question was suggested to the writers by the observation of an anterior urethritis in a man, apparently caused by the Hoffmann type of the pseudodiphtheria bacillus. The same bacillus also was discovered in the cervical canal of the patient's wife, sterile in spite of intravaginal medication, dilatation and curettage, after seven years of married life. Three months after a vaccine treatment had been finished, she became pregnant. In two other cases of female sterility they found in the one a pure culture of diphtheroids, in the other a mixed growth in which the diphtheroids predominated. The writers do not answer the question asked in the title in the affirmative, but feel that these findings should encourage further study.